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## THE PSYCHOLOGICAL REVIEW

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### EJECTIVE CONSCIOUSNESS AS A FUNDAMENTAL FACTOR IN SOCIAL PSYCHOLOGY<sup>1</sup>

BY MARGARET FLOY WASHBURN

*Vassar College*

Like sociology, social psychology has been stronger in description than in explanation. At its service for explaining the phenomena of human social behavior it has three classes of psychological influences. Two of these are universally recognized: first, the great social drives—gregarious, fighting, self-exhibiting, imitating, submitting, mating, and protecting; and secondly, the laws of learning. The third influence is probably taken for granted by everyone, but its working has never been adequately analyzed and described in print. This influence is that of ejective consciousness, and ejective consciousness is one's idea of what is going on in other minds. It was W. K. Clifford who coined the word 'eject' to designate an idea representing a state in another person's mind (2). Ejective consciousness does not necessarily involve sympathy. We can contemplate what we suppose to be another person's state of mind, without sharing it; we may feel towards it attitudes ranging from sympathy through indifference to violent antagonism. It should also be stated at the outset of this address that when I speak of ejective consciousness as influencing social behavior, I am not implying interaction between body and mind. The expression is used as an abbreviation: the full statement to be understood is that

<sup>1</sup> Address given by the honorary President at the meeting of the New York Branch of the American Psychological Association at Philadelphia, April 9, 1932.

social behavior is influenced by the neuromuscular processes which accompany ejective consciousness.

The presence of ejective consciousness in man explains the most striking differences between his social behavior and that of the lower animals. Animal social behavior results from the combined influence of external stimuli, especially though not exclusively those stimuli resulting from the behavior of other animals; and internal rhythms, which at certain seasons intensify fighting, mating, and parental behavior. When the external stimulus or the internal rhythm is lacking, animal social behavior lapses. Many observations show that it is unaccompanied by any ideas of the thoughts or feelings of other animals, for instance Herrick's observation (4) that nestlings of a late summer brood are abandoned when the autumn migratory drive begins. Man through his power of imagining the thoughts and feelings of others can keep his social behavior constant though the external stimuli and the internal rhythms both vary. This idea was expressed in an article I wrote for the Titchener Commemorative Volume, published in 1917, and the same article traced certain features of the course of development of ejective consciousness, which as it grows broadens in its space and time references and passes from the power of interpreting the emotions of others to the power of interpreting their ideas, and from the power of imagining that others think and feel as we do to the power of realizing that they think and feel differently from ourselves.

What I wish to add to these ideas which have already been published may be outlined as follows.

First, ejective consciousness explains certain features of the social and moral sentiments. Secondly, it is a necessary concept in explaining the difference between normal and abnormal suggestibility. Thirdly, it constitutes the difference between the religious and scientific views of the world. Fourthly, it is essential to the very definition of language and makes the difference between language and involuntary emotional expression. Fifthly, it is the essence of the creative impulse in art, and deeply involved in the enjoyment of art. And lastly, it is fundamental to our sense of the comic.

The statement that our social sentiments and emotions depend on what we think is going on in other people's minds needs little discussion; its truth is obvious. We like people when we imagine that they share our own ideas and feelings; we like them intensely when we see a favorable image of ourselves floating about in their minds. Nothing arouses the fighting drive so strongly as the thought of undeserved contempt for us harbored in another mind, or more completely annihilates the self-exhibiting drive in us as the realization that we deserve the contempt we read in the mind of others. The important temperamental difference between a vain person and a conceited person is entirely dependent on a difference in what we may call the limen of ejective consciousness. Both are egocentric, but for the vain person the limen is low; he is always getting his feelings hurt and making ejective inferences as to the presence of insufficiently admiring reflections of himself in other minds. The conceited person, with a high threshold of ejective processes, is comfortably wrapped in a coating of imperviousness to all suggestions from the behavior of his fellow-beings that could indicate the presence in their minds of anything but a favorable image of himself. As for the moral sentiments, Adam Smith, the great author of *The Wealth of Nations*, gave an account of them that is psychologically sound: we disapprove of another's conduct when we fail to sympathize with the motives we read in his mind, and are depressed by our realization of the sufferings he is causing in the minds of his victims. We approve of it when we sympathize with his motives and with the happiness he causes. As for conscience, our approval or disapproval of ourselves, no one has given a better psychological account of it than the same writer when he defined it as sympathy with the judgment of an imaginary impartial spectator of our own conduct. Speaking of the conscientious man, Smith said, "He has never dared to forget for one moment the judgment which the impartial spectator would pass upon his sentiments and conduct. He has never dared to suffer the man within the breast to be absent one moment from his attention. With the eyes of this great inmate he

has always been accustomed to regard whatever relates to himself" (8).

If any concept can be regarded as a foundation stone of social psychology it is suggestibility. Sidis's laws of normal and abnormal suggestibility, namely, that normal suggestibility varies inversely with the directness of the suggestion and abnormal suggestibility varies directly with the directness of the suggestion (7), can be explained only by the presence of ejective consciousness in the normally suggestible person and its absence in the abnormally suggestible person. Why do we in the normal state tend to resist a direct suggestion? Because when someone gives us a direct command we infer that in this individual's mind there exists the idea of us as inferior to himself. If we know ourselves to be his inferior, that is, if he has prestige, we may accept the suggestion. Otherwise he has to use indirect suggestion, making it appear that we thought of the suggested act ourselves, or a contrary suggestion, which we assert our superiority by resisting. Abnormal suggestibility, as in the hypnotic trance, when direct suggestions are accepted without resistance, occurs when through dissociation ejective consciousness is absent and we cannot think about what is in the suggester's mind. What is there about a psychological crowd that makes it so suggestible? *Not* dissociation, but a special form of ejective consciousness, the sense that everyone is thinking and feeling as we do at the moment. Under normal conditions we are always more or less aware of the criticism of others; this is a form of ejective consciousness that inhibits action and forces us to think, if we own anything to think with. In the crowd, when 'the gang's all here' another type of ejective consciousness appears, in which our ideas and feelings are felt to be bigger than ourselves and our strength is as the strength of the gang. This type is invaluable to action, as when soldiers go into battle, but fatal to thought.

Thirdly, ejective consciousness is contained in the very essence of religion; not that here as in the moral sentiments we sympathize with our fellow-beings, but that we sympathize with God. The attempts to define religion are endless. To

base a definition on the derivation of the word, that which binds us, or (to use the modern term) inhibits us, is to include under religion a heterogeneous collection of factors in human nature from Freudian complexes up. The best definition of religion in my opinion rests on the fact that there is a fundamental opposition between the religious and the scientific attitudes toward the world. The latter regards the world unejectively, as the expression of impersonal forces which can be understood but not ejectively interpreted. The religious attitude on the other hand regards the world as the expression of personal forces with which social relations may be maintained. It is a fascinating task for the social psychologist to trace the interplay of these opposite tendencies in man's attempts to control his environment through the ages. There are the rituals of sympathetic magic based on the genuinely scientific, because impersonal, principle that you can make a thing happen by imitating it; the only defect of this principle is that it doesn't happen to be true. The rituals which propitiate the gods display that rudimentary ejective consciousness which is expressed in fear. The rituals of the higher religions are addressed to gods made in the full image of man and therefore loved. How hard it is for man to take a consistently scientific or impersonal view of the universe is strikingly shown at the present time, when sound experimenters in physical science emerge from their laboratories and tell us that they find there consoling evidence at one and the same time of an Infinite Mind in the cosmic order and of man's free will in the unpredictable vagaries of electrons.

Fourthly, two great instruments of ejective consciousness are language and art. If to define religion as that attitude towards the universe which regards it as an expression of personal forces has at least the merit of being a clear conception, so the merit of clearness, and I believe also the merit of truth, attach to the definition of language as a system of sounds or movements used with the intention to produce states in other minds. This makes the presence of ejective consciousness differentiate language from the involuntary expressions of emotion, and also from sounds and movements used intentionally because animals have learned that they

produce certain behavior in others. Three degrees can be traced here. First, a dog or a man may howl from pain, involuntarily, because the preformed outlet of the excess energy set free in the emergency leads to the vocal organs. Secondly, a dog may howl in order to be let in, because his individual experience is that the door opens when he howls. The third grade is probably above the dog's intellectual level, though I am not too sure of this, but a neurotic human being may howl for the pleasure of imagining himself an object of at least momentary importance in the minds of his companions; he has the blessed gift of ejective consciousness and his howling is true language. The development of language of course bears everywhere indications that it is addressed not alone to the behavior of others but more directly to their thoughts and feelings. For example there is an interesting law of meaning change which is a conspicuous example of the combined influence of ejective consciousness, the self-exhibiting drive, and the law of affective conditioning. I refer to the tendency which the older philologists called the natural depravity of words; the tendency for a word with originally a virtuous meaning to lose it and take on a vicious meaning, as 'lewd' originally meant 'not clerical, lay,' and 'pirate' originally meant 'an enterprising person.' These changes involve, first and secondly, the self-exhibiting drive and ejective consciousness; and thirdly, the law of affective spread or transfer. Under the influence of the self-exhibiting drive, we wish to contemplate, with the aid of ejective consciousness, a favorable idea of ourselves in the mind of others. But experience has taught us that the process of affective conditioning will bring it about that if we mention improper things to our neighbors impropriety will attach itself to our own image in their minds. Therefore we call the improper thing by a harmless name and the harmless name in course of time acquires a bad character and must in its turn be avoided.

As for art, Hirn in his *Origins of Art* showed that the creative impulse is in its essence ejective; it is the desire of the artist, not so much to get material reward but rather to share his vision with other minds. "The work of art," Hirn says, "presents itself as the most effective means by which the

individual is enabled to convey to wider and wider circles of sympathizers an emotional state similar to that by which he is himself dominated" (5). The artist wants to make permanent this sharing by others of his vision, and hence he seeks a permanent material to embody the vision; if the material has to be perishable, he may be consoled if great numbers of persons are reached in the short time of its duration. Into the enjoyment of art, as distinguished from its creation, ejective consciousness of course enters in many ways, ranging from the rudimentary ejective consciousness involved in empathy, when we project our own kinesthetic sensations into spatial design, think of a Gothic arch as soaring and sympathize with the discomfort of a top-heavy structure, to the abnormally complete ejective consciousness produced by a powerful drama, where our whole mind is filled with the emotions and thoughts which we ascribe to the characters, and we forget all our own affairs as we never do in real life. The difference between the plot novel and the psychological novel lies in the degree of ejective consciousness called forth in the reader; in the plot novel he uses only rudimentary interpretations, which lead to his regarding the hero and heroine merely as the representatives of virtue in distress, and withholding all sympathy from the villain. On the other hand, the test of a good psychological novel is that the reader enters sympathetically into the thoughts and feelings of every character in the book.

Finally, the comic is a field that belongs to social psychology, not merely because a good joke needs to be shared or because ridicule is a method of warfare, but because ejective consciousness is its very essence. The comic, of course, always involves incongruity, but it is the incongruities of human behavior and motives. It was Bergson, I think, who first pointed out that nothing is funny but people. "The comic," he says, "does not exist outside the pale of what is strictly human" (1); if we find animals or even inanimate objects funny, it is because we read human feelings into them. What differentiates the ejective consciousness that enters into the experience of the comic from full sympathetic interpretation of another mind, is that it is incomplete. When a

pompous individual is disconcerted, if we could not read his mind and realize his exaggerated idea of his own importance we should not find him comic; if we completely read his mind and realized his feelings of humiliation we should not find him comic. "Laughter," said Hobbes (6), "is sudden glory." To my own way of thinking, it must have originated in the shouts sent after a defeated foe. Freud points out that the sense of humor is a mechanism for economizing sympathetic emotion (3). Surely its essence is a type of ejective consciousness that sees what fools these mortals be, but shuts its eyes to their shame and grief at their own folly.

These examples of the functioning of ejective consciousness, I hope, tend to show the inadequacy of a social psychology that would define social behavior in man as reaction to the *behavior* of his fellow-men, rather than reaction also to what he conceives to be the *mental states* of others. In none of these experiences—social, moral, and religious sentiments, suggestion, language, art, and the comic—are we reacting merely to what our fellow-men do, or even to what we expect them to do. If we know that we are contemptible in the eyes of another person whose opinion we respect, we are indignant or unhappy even though we know also that he will not harm us by word or act. Any creative thinker is made happy by finding that his work is approved by a competent critic even though no possible practical benefit may result to him. Insults lie not in words or acts but in what is seen to lie behind them in the mind. Not only as a man acts, but 'as he thinketh in his heart,' so is he, and it is to his thought, even more than to his actions, that we adjust our behavior.

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## SO-CALLED GROUP FACTORS AS DETERMINERS OF ABILITIES

BY R. C. TRYON

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### I. INTRODUCTION

In the preceding paper (18) ten experiments dealing with the interrelation between abilities were examined by tetrad analysis, and the conclusion was drawn that no good evidence existed to support Spearman's theory of two factors. It was further concluded that a theory postulating multiple factors is demanded. Far from shaking belief in the two factors, such evidence as this seems to elicit from the proponent of two factors reiterated proclamations of the virtues of the theory (13). By postulating a host of group factors or 'disturbers,' the two-factorist attempts to 'explain' the aberrancy of these data. But just why one should be disposed even to entertain the basic unadulterated theory when no crucial evidence supports it should be the initial question rising to one's mind in his contemplation of the problem of the causes of abilities.

#### *A. Group Factors Postulated by the Two-Factorists*

As a consequence of the complete failure of the accumulating data to support the pure two factor theory, the list of group factors or 'disturbers' which the two-factorist has had to put forth has grown to such a length that the theory threatens to become a multiple factor theory in its own right. If the two-factorist would only quit his insistence upon a monarchic universal *g*, but retain the innumerable group factors and 'disturbers,' which have considerably more definiteness and intelligibility than the mystical *g*, the multiple-factorists would consider him a desirable candidate for membership in their society. The writer has gleaned from the writings of Spearman and other members of his school,

some forty-one group factors, or faculties, or 'disturbers,' which they have advanced as actual or potential partial determiners of individual differences in mental abilities. Table I gives this list, which also includes other 'general' factors than  $g$ . I have divided the list into three classes, (1) those factors which may be considered as clear-cut psychological faculties, (2) those 'disturbers' of tetrads (*i.e.*, those which are capable of causing the tetrads to fail to vanish) which depend upon psychological factors and which mediate them, and (3) certain measurement factors which tend to generate or to simulate group factors. Following each factor I have appended a reference to two factor literature in which the given factor is postulated.

#### *B. The Purification Technique*

With this imposing array of subsidiary factors at hand, the two-factorist is undisturbed by the fact that some of the tetrad differences are significantly too large. For this embarrassing situation, he has devised a supplementary technique, which we may divide into the following steps: *First*, he observes those tetrads which are too large, *second*, he notes the  $r$  which seems to occasion the too large values of these tetrads, and *third*, he 'analyses' the two variables between which this  $r$  is determined, endeavoring to discover to which one of the above forty-one factors a 'specificality' or 'special overlap' may be assigned. It is inevitable that he find among the forty-one, one which by dint of much dialectics may seem to be reasonable. *Fourth*, on the ground that here is a group factor which is a 'disturber' of the tetrads, he throws out either all of the tetrads involving the guilty  $r$ , or, more inclusively, he casts out all of the tetrads involving one of the variables possessing specificity. *Fifth*, he continues this process of purification of the tetrads until only those tetrads are left which vanish within expectations of sampling. Then, *sixth*, he solemnly declares the variables left to be determined by the two factors, for the tetrad difference criterion for these is satisfied.

There are, however, several illogical and unconvincing

TABLE I

## LIST OF POTENTIAL OR ACTUAL FACTORS (OR 'DISTURBERS') POSTULATED BY TWO-FACTORISTS

1. *Direct Psychological Factors or Faculties*

- (1) Logical ability (10, p. 226)
- (2) Constructive mechanical ability (10, p. 229)
- (3) Arithmetic ability (10, p. 232)
- (4) Geometrical ability (10, p. 232)
- (5) Social behavior (10, p. 233)
- (6) Reaction-time (10, p. 236)
- (7) Visual-auditory imagery (10, p. 238)
- (8) Music appreciation (10, p. 242)
- (9) Memorization in unlike functions (10, p. 286)
- (10) Sensory memory (10, p. 287)
- (11) Verbal memory (10, p. 288)
- (12) Non-verbal symbolic memory (10, p. 288)
- (13) General perseveration (10, Chap. 17)
- (14) General subjective fatigue (10, Chap. 18)
- (15) General oscillation (10, Chap. 19)
- (16) General purposive consistency or self-control (10, Chap. 20)
- (17) General freedom from inertia (10, Chap. 20)
- (18) Speed preference (10, p. 155, 14, pp. 180ff., 15, p. 261)
- (19) Conative inhibition (16, p. 343)
- (20) Verbality (16, pp. 344ff.)
- (21) Ability to keep test unit before 'mind's eye' (16, p. 347)

2. *Indirect Psychological Factors or 'Disturbers'*

- (22) Maturity or age (10, p. 155)
- (23) Training and education (10, p. 155)
- (24) Sex (10, p. 155)
- (25) Plurality of experimenters (10, p. 156, 12, p. 564)
- (26) Correlated training, as between mathematics and physics (12, p. 562)
- (27) Race (12, p. 562)
- (28) Idiosyncrasy due to test constructors (12, p. 564, 15, p. 260)
- (29) Propinquity of tests (12, p. 564, 15, p. 262)
- (30) Similarity of fundaments (2, p. 41)
- (31) Similarity of relations (15, p. 259)
- (32) Similarity of form (10, p. 153, 16, p. 335)
- (33) Novelty, or 'shock' (15, p. 257, 16, p. 343)
- (34) 'Not-clear-understanding of the test requirements' (16, p. 343)
- (35) Forepractice (16, p. 343)
- (36) Group testing (15, p. 263)
- (37) School and class (15, p. 264)

3. *Measurement Factors*

- (38) Miscalculations (10, p. 157, 12, p. 565)
- (39) Sampling errors (10, p. 140, 12, p. 561)
- (40) Spurious  $r$  between ability rating, e.g., 'halo' (10, p. 151)
- (41) Scale anomalies (12, p. 565ff., 14, pp. 176ff.)

features in this purification technique. The first is that it smacks of the fallacy of being wise after the event. The post-mortem psychological 'analysis' of looking for one of the forty-one group factors or disturbers which 'explain' the excess in the tetrads is only instituted *after* it is discovered that certain tetrads fail to vanish properly. This *a posteriori*

form of 'analysis' (typical of the thinking in the social sciences in general) is not particularly objectionable provided it is extended to *all* the variables, and leads to psychological consistency. If it is noted, for example, that  $r_{24}$  inevitably occurs in tetrads which are significantly too large, and a psychological analysis divulges that  $x_2$  and  $x_4$  are both tests involving, say, 'verbal memory,' then before one may decide that verbal memory represents a special group factor in the two factor sense, one must *also* show that such a mnemonic ability is *not* apparent among other variables which give  $r$ 's that *do* satisfy the criterion. If a psychological analysis shows that some tests involving verbal memory give  $r$ 's which satisfy the criterion, while other tests likewise involving verbal memory do not satisfy the criterion, then verbal memory cannot be put forth as a 'group factor' in the two factor sense of its being a special source of correlation other than  $g$ . But if, on the other hand,  $r$ 's between tests involving verbal memory *never* occur in tetrads which vanish, but always occasion tetrads which contain them to take too large values, then 'verbal memory' may reasonably be put forth as a group factor in the two factor sense. This type of *psychological consistency*, namely, of extending the psychological analysis to all of the variables intercorrelated, and showing that the results of such analysis are rigorously consistent with the behavior of the tetrads is not considered a requirement in two factor analysis, which is extended no farther than to the variables whose  $r$ 's lead to excessively large tetrads.

Another unconvincing feature is that the tetrads which are thrown out are rarely analysed for *statistical consistency* with the theory of overlap which is postulated. The entire statistical analysis is usually confined to the remaining unexcluded tetrads which are examined to see if they satisfy the criterion. It should be apparent to anyone that *of course* the remaining tetrads satisfy the criterion, simply because aberrant tetrads are thrown out until those remaining *do* satisfy it.

Trouble arises, however, when one examines the aberrant tetrads themselves. The values which they take must behave

in certain systematic ways if the postulated theory of overlap is correct. In any actual problem, if one takes the trouble to examine these tetrads, one generally finds, however, that they take values *not* consistent with the theory. Those which, on the theory of overlap postulated, should not vanish, often do; those which should take positive signs often take negative, and *vice versa*, and it becomes clear that the theory of overlap is not proved.

A final feature of this purification technique is that it constitutes no proof or disproof of the two factor theory. It seems, in fact, to be without crucial significance, for it may mean (as the two-factorist insists) that the unexcluded tetrads which *do* satisfy the criterion refer to abilities determined by *g* and the *s*'s, but it may on the other hand mean (as the multiple-factorist is inclined to believe) that the process of purification has simply weeded out those variables, multiple factorially determined, whose factor patterns do not result in the happy condition of balance as described in the previous paper in equation (9), leaving only those which do. Indeed, since the criterion had not been satisfied in the first place, thus demanding the postulation of multiple group factors, this second explanation of the effect of purification would seem most tenable.

## II. EFFECT OF GROUP FACTORS ON THE TETRADS

In the previous paper we saw clearly that the distribution of tetrad differences in every investigation showed many tetrads significantly greater than zero. When the problem was one investigated by a two factor enthusiast, the investigator usually warped such data into conformity with his theory by postulating several group factors, and by 'purifying' the data as described above. In the present paper, we are going to examine some of these discarded tetrads, to the end of observing whether they take values psychologically and statistically consistent with the modified two factor theory as postulated.

To conduct this analysis briefly, we shall make use of the *T* notation described by the writer in the previous paper (18,

section IV) in which  $T_{abcd}$  stands for the three tetrads:  $T'_{abcd} = t_{abcd}$ ,  $T''_{abcd} = t_{abdc}$ , and  $T'''_{abcd} = t_{acdb}$ . We must first observe the effect of factors of different orders upon the form of the triplet-set of tetrads.<sup>1</sup>

#### A. Expected Forms of $T$ on the Assumption of $g$

Let us consider the familiar two factor pattern:

$$\begin{aligned} x_1 &= f(a, s_1) & x_3 &= f(a, s_3) \\ x_2 &= f(a, s_2) & x_4 &= f(a, s_4) \end{aligned} \quad (1)$$

in which  $a$  (*i. e.*,  $g$ ) is the third order factor ('general' here, since common to the four variables), and the  $s$ 's are zero order factors specific to each. In this case,

$$T_{1234} \begin{cases} T' = t_{1234} = \alpha_1\alpha_2\alpha_3\alpha_4 - \alpha_1\alpha_3\alpha_2\alpha_4 = 0 \\ T'' = t_{1243} = \alpha_1\alpha_2\alpha_4\alpha_3 - \alpha_1\alpha_4\alpha_2\alpha_3 = 0 \\ T''' = t_{1342} = \alpha_1\alpha_3\alpha_4\alpha_2 - \alpha_1\alpha_4\alpha_3\alpha_2 = 0 \end{cases} \quad (2)$$

in which  $\alpha_i$  is the coefficient of  $a$  in  $x_i$  ( $i = 1, 2, 3, 4$ ) as previously described (18, section II).

Now, suppose, however, that besides the factors in (1), a first order group factor,  $b$ , is common to  $x_1$  and  $x_2$ , thus:

$$\begin{aligned} x_1 &= f(a, b, s_1) & x_3 &= f(a, s_3) \\ x_2 &= f(a, b, s_2) & x_4 &= f(a, s_4) \end{aligned} \quad (3)$$

The result in the tetrads, according to Garnett's formula, see 18, formula (7), is:

$$T_{1234} \begin{cases} T' = (\alpha_1\alpha_2 + \beta_1\beta_2)\alpha_3\alpha_4 - \alpha_1\alpha_3\alpha_2\alpha_4 = \alpha_3\alpha_4\beta_1\beta_2 \\ T'' = (\alpha_1\alpha_2 + \beta_1\beta_2)\alpha_4\alpha_3 - \alpha_1\alpha_4\alpha_2\alpha_3 = \alpha_4\alpha_3\beta_1\beta_2 \\ T''' = \alpha_1\alpha_3\alpha_4\alpha_2 - \alpha_1\alpha_4\alpha_3\alpha_2 = 0 \end{cases} \quad (4)$$

in which  $\beta_i$  is the coefficient of  $b$  in  $x_i$  ( $i = 1, 2$ ). Assuming that all coefficients are positive, then the effect of such a group factor in  $x_1$  and  $x_2$  is to make  $T' = T'' =$  a positive value greater than zero, but  $T''' = 0$ . Note that the same effect

<sup>1</sup> The 'order' of a group factor defines the number of variables in which it occurs. Thus a zero order factor occurs in one variable and *no* others, a 1st order factor occurs in one variable and *one* other, a 2d order factor occurs in one variable and *two* others, . . . an  $(n - 1)$ th order factor occurs in  $n$  variables, *i.e.*, is general.

occurs when the special factor occurs in  $x_3$  and  $x_4$ , instead of  $x_1$  and  $x_2$ , that is, if

$$\begin{aligned} x_1 &= f(a, s_1) & x_3 &= f(a, b, s_3) \\ x_2 &= f(a, s_2) & x_4 &= f(a, b, s_4) \end{aligned} \quad (5)$$

then

$$T_{1234} \begin{cases} T' = \alpha_1\alpha_2(\alpha_3\alpha_4 + \beta_3\beta_4) - \alpha_1\alpha_3\alpha_2\alpha_4 = \alpha_1\alpha_2\beta_3\beta_4 \\ T'' = \alpha_1\alpha_2(\alpha_4\alpha_3 + \beta_4\beta_3) - \alpha_1\alpha_4\alpha_2\alpha_3 = \alpha_1\alpha_2\beta_4\beta_3 \\ T''' = \alpha_1\alpha_3\alpha_4\alpha_2 - \alpha_1\alpha_4\alpha_3\alpha_2 = 0 \end{cases} \quad (6)$$

But if the group factor,  $b$ , occurs in  $x_1$  and  $x_3$ , and in no others, the result is different, to wit:

$$T_{1234} \begin{cases} T' = \alpha_1\alpha_2\alpha_3\alpha_4 - (\alpha_1\alpha_3 + \beta_1\beta_3)\alpha_2\alpha_4 = -\alpha_2\alpha_4\beta_1\beta_3 \\ T'' = \alpha_1\alpha_2\alpha_4\alpha_3 - \alpha_1\alpha_4\alpha_2\alpha_3 = 0 \\ T''' = (\alpha_1\alpha_3 + \beta_1\beta_3)\alpha_4\alpha_2 - \alpha_1\alpha_4\alpha_3\alpha_2 = \alpha_4\alpha_2\beta_1\beta_3 \end{cases} \quad (7)$$

Note here that  $|T'| = |T'''|$ , but  $T' < 0 < T'''$ , and  $T'' = 0$ . The same effect occurs if the group factor occurs in  $x_2$  and  $x_4$  instead of  $x_1$  and  $x_3$ .

Finally, if the special factor,  $b$ , is in  $x_1$  and  $x_4$  and in no others, the result is:

$$T_{1234} \begin{cases} T' = \alpha_1\alpha_2\alpha_3\alpha_4 - \alpha_1\alpha_3\alpha_2\alpha_4 = 0 \\ T'' = \alpha_1\alpha_2\alpha_4\alpha_3 - (\alpha_1\alpha_4 + \beta_1\beta_4)\alpha_2\alpha_3 = -\alpha_2\alpha_3\beta_1\beta_4 \\ T''' = \alpha_1\alpha_3\alpha_4\alpha_2 - (\alpha_1\alpha_4 + \beta_1\beta_4)\alpha_3\alpha_2 = -\alpha_3\alpha_2\beta_1\beta_4 \end{cases} \quad (8)$$

Here  $T'' = T'''$ , a negative value, whereas  $T' = 0$ . And the same effect is secured if the group factor occurs in  $x_2$  and  $x_3$ , instead of  $x_1$  and  $x_4$ .

If we let the presence of such products as  $\beta_1\beta_2$  occurring on the left side of the minus sign in a given tetrad difference be indicated in the resulting difference by '+', and let '-' indicate such a product occurring on the right side, then the above consequences of a group factor on tetrads may be symbolized as follows:

1st order factor in:	$T'_{1234}$	$T''_{1234}$	$T'''_{1234}$	Type
(12).....	+	+	0	A
(34).....	+	0	0	A
(13).....	-	0	+	B
(24).....	-	0	+	B
(14).....	0	-	-	C
(23).....	0	-	-	C

In an  $n$  variable problem, if an experimenter postulates a group factor between, say,  $x_2$  and  $x_7$ , then all triplet-set  $T$ 's of the form  $T_{2ij7}$  ( $i, j = 3, 4, 5, 6, 8, \dots n$ ;  $i$  and  $j$  never having the same value in the same  $T$ ), than all such  $T$ 's should be of type C.  $T$ 's of the form  $T_{12i7}$  ( $i = 3, 4, 5, 6, 8, \dots n$ ) should all be of type B, etc.

Experimenters are sometimes wont to postulate two or more 1st order group factors. This complicates the results in the tetrads. For instance, if the pattern is:

$$\begin{array}{ll} x_1 = f(a, b, s_1) & x_3 = f(a, c, s_3) \\ x_2 = f(a, b, s_2) & x_4 = f(a, c, s_4) \end{array} \quad (10)$$

the result would be:

$$\begin{aligned} T' &= (\alpha_1\alpha_2 + \beta_1\beta_2)(\alpha_3\alpha_4 + \gamma_3\gamma_4) - \alpha_1\alpha_3\alpha_2\alpha_4 \\ &= \alpha_1\alpha_2\gamma_3\gamma_4 + \alpha_3\alpha_4\beta_1\beta_2 + \beta_1\beta_2\gamma_3\gamma_4 \\ T'' &= (\alpha_1\alpha_2 + \beta_1\beta_2)(\alpha_4\alpha_3 + \gamma_4\gamma_3) - \alpha_1\alpha_4\alpha_3\alpha_2 \\ &= \alpha_1\alpha_2\gamma_4\gamma_3 + \alpha_3\alpha_4\beta_1\beta_2 + \beta_1\beta_2\gamma_4\gamma_3 \\ T''' &= \alpha_1\alpha_3\alpha_4\alpha_2 - \alpha_1\alpha_4\alpha_2\alpha_3 = 0 \end{aligned} \quad (11)$$

Thus the effect is systematically the same as in the type A tetrads in (9). If one will compare  $T'$  and  $T''$  of, say, (4) with those of (11) it will be apparent that in the first case these tetrads take positive values because of the presence of the term,  $\beta_1\beta_2$ , to the left of the minus sign, but in the last case they take positive values because of the presence of *two* such terms, namely,  $\beta_1\beta_2$  and  $\gamma_3\gamma_4$ . Again, if we symbolize every such extra product term to the left of the minus sign as having a plus or '+' effect on the resulting tetrad difference and every such extra product term to the *right* of the minus sign as having a minus or '−' effect, then we may symbolize the difference between the type A and type AA tetrad as follows:

1st order factor in:	$T'_{1234}$	$T''_{1234}$	$T'''_{1234}$	Type
(12) or (34).....	+	+	o	A
(12) and (34).....	++	++	o	AA

Thus, while the systematic form of the triplet-set in type A and AA is the same, in the latter type the magnitudes of the first two tetrads should be greater than in the former, other things being equal.

Analogous effects result when a 1st order factor occurs in (13) and another in (24), for since each such single factor results in type B, the two acting together result in type BB. Likewise, special linkage in (14) and also in (23) result in a type CC triplet-set. These cases would be symbolized, as follows:

1st order factors in:	$T'_{1234}$	$T''_{1234}$	$T'''_{1234}$	Type
(13)(24).....	--	oo	++	BB
(14)(23).....	oo	--	--	CC

Hence, if a group factor is present which gives a type A result, the presence of another factor also leading to a type A, results in the two working together to form a type AA triplet-set. As a consequence of this cumulative effect, when a group factor leading to, say, type B occurs in the same variables in which another group factor is present but leading to a type A result, a cancelling effect will result. Thus a special 1st order link in (12) and another in (13) would lead to the following result:

$T'_{1234}$	$T''_{1234}$	$T'''_{1234}$		$T'_{134}$	$T''_{1234}$	$T'''_{1234}$
+-	+o	o+	Or approximately	o (?)	+	+

The same objective result in the triple-set would be secured if there were 1st order factors in (12)(24); (13)(34); or (24)(34).

Without going into further detailed extensions of this reasoning, we may summarize the expected effects on the three tetrads of the operation of group factors of 1st, 2d and 3d orders. This is done in Table II. In the first column of this table, various types of group factors of different orders are postulated, and the expected effects on the three tetrad

differences are given in columns to the right. The different patterns leading to the same result are given in the same row.

In section *i*, I to VI, are given all the factor patterns involving one or more 1st order group factors. Thus II<sup>d</sup> shows a factor pattern involving two 1st order factors, one in the common factor (12), the other in (13). The expected effects in each tetrad difference,  $T'_{1234}$ ,  $T''_{1234}$ , and  $T'''_{1234}$ , according to the full '+' and '-' notation described above are shown under 'Full expectation,' and the full-lettered triplet-set type expected is given in the fourth column under 'Full expectation.' Under 'Approximate expectation' are shown the expected effects taking cognizance of the cumulative effects, it being anticipated that, for example, '+-' under  $T'_{1234}$  of 'Full expectation' will reduce, other things being equal, to approximately '0' as expressed under  $T'_{1234}$  in 'Approximate expectation,' the question mark indicating that exactly zero may not be recovered. In this case it is to be noted that three other linkage patterns (separated by semi-colons) besides (12)(13), namely, (12)(24); (34)(13); (34)(24), each involving two 1st order factors, result in the same full expected and approximate form. Since the final approximate form results in the first tetrad difference being '0', and the second and third being '+', we have designated this new systematic approximate form, type D, in the next to last column. As there are four patterns leading to this result, in the last column the number '4' under 'Total' indicates this fact. To take another example, note that in III<sup>g</sup> there are eight factor patterns involving three 1st order group factors each, and that all lead to full expectation type ABC, which reduces to the approximate form of all three tetrads zero. This approximate form has been designated type M.

Under section *ii* there are presented all patterns involving group factors of the second order. For example, under VII, we note one as being (123), which means a group factor running through  $x_1$ ,  $x_2$  and  $x_3$ . There are four such kinds of linkages, namely, (123); (124); (134); and (234); and all lead to vanishing tetrad differences of type M. Note that under

section *ii*, the presence of one or more 2d order factors results in type M, and that there are a total of 15 patterns which may lead to this result.

Under section *iii* is the one pattern involving a 3d order group factor (*i.e.*, general as far as the four variables under consideration are concerned). Under sections *iv* to *vii* are indicated the expected results of linkage patterns involving all combinations of 1st, 2d and 3d order factors.

The summary table gives the total number of triplet-sets of each approximate type from each section above. Thus, in the row labelled '1st order' we see that there are six linkage patterns involving one or more 1st order factors (from section *i*) which result in the same approximate type A, and that there are nine linkage patterns involving 1st order factors which may result in vanishing tetrads, Type M. Note that from section *iv*, from which the results in the row labelled '1st and 2d order factors' are taken, there are 135 different linkage patterns, each involving 1st and 2d order group factors, and each resulting in type M, or vanishing tetrad differences. In the bottom row, we see that, considering all classes of factor linkages, there are 319 linkage patterns involving group factors which may result in vanishing tetrads, that there are 192 ways of producing types A to F, 64 ways of producing types G to L, and 32 ways of producing AA to FF.

This table does not ostensibly take into consideration repeated factors, *i.e.*, group factors of the same order and type of linkage. For example, we seem not to have considered a linkage pattern involving two different 1st order factors between  $x_1$  and  $x_2$ . In reality, we have included such instances, for we have simply meant by a linkage in (12) all first order factors taken together which specially overlap  $x_1$  and  $x_2$ , and extend to no other of the variables. They are functional unities only relative to the set-up of variables under consideration. Such repeated factors serve apparently only to increase the magnitude of such extra terms as  $\beta_1\beta_2$  in the tetrad differences.

The writer does not wish to imply that tetrad differences

will always take clear-cut forms from types A to FF. The forms taken by tetrads will be sensibly affected by the relative magnitudes of such product terms as  $\beta_1\beta_2$ ,  $\gamma_3\gamma_4$ , etc. This table has usefulness, however, in showing what forms the tetrad differences may take under certain pattern postulations, and further in showing that the same types may result from quite different linkage patterns.

We shall make extensive reference to this table in considering actual data. If an experimenter, for example, postulates one 1st order factor between, say,  $x_3$  and  $x_5$ , in an  $n$  variable problem, throwing out all tetrads involving  $r_{35}$ , we shall examine these discarded tetrads to see if they conform to expectation as shown in this table. Thus, we should expect  $T_{1235}$  to show the approximate form of type A,  $T_{2345}$  to show type B,  $T_{2356}$  to show type C, etc. If the discarded tetrads do not conform to expectation, we shall consider the group factor postulated as not proved consistent with the objective results; if they do conform we will accept it as consistent. If it becomes necessary (as it frequently does) for an experimenter to postulate several group factors of different orders in order to warp the observed data into an adulterated two factor system, say, to postulate a 1st order factor between  $x_1$  and  $x_2$ , and a 2d order factor between  $x_1$ ,  $x_3$  and  $x_4$ , we should expect the resultant  $T_{1234}$  to take a form such as type A,  $T_{1345}$  to take the form type M, etc.

#### *Irreversibility of the Argument*

The unfortunate fact appears, as it does in *all* factor analysis, that one cannot *uniquely* reason back from tetrad differences to patterns. Suppose that one actually finds that  $T_{1234}$  is of type A. This does not mean that there is clearly a link between (12) and/or (34), and that no other group factors may be postulated. It may mean, according to our summary table in Table II, that the group factor pattern may be any one of the possible 192. The instances in which one finds the tetrads actually to vanish may be 'explained' in any one of the 319 ways indicated in the

summary table, and each of these involves the presence of group factors.

*B. Expected Forms on the Assumption of Multiple Factors Without a g*

Having now observed the effects of group factors associated with the two factors,  $g$  plus  $s$ , we might well examine the effects to be expected assuming no  $g$  but multiple group factors only. The amazing fact appears that if we postulate *at least* six 1st order group factors *or* two 2d order factors *or* one 3d order factor among the four variables, or any combinations of these three kinds of factor linkages, the additional postulation of just those group factors indicated in the extreme left column of Table II will lead to the very same types of tetrads as indicated under 'Approximate expectation.' Thus the postulation in (12) of an additional 1st order factor, besides the ones mentioned just above, would result in approximate type A; similarly, an additional postulation of 1st order factors in, say, (12) (34) (13) would result in approximate type G, etc.

As the writer envisages the multiple factors that determine individual differences in abilities and the correlations between them, such factors would not preponderantly present such simple types as A, B, C, and M. Since each ability is considered to be the result of the operation of a multiplicity of genetic and learning factors brought into expression by the experimental situation in which the ability is measured, the intercorrelations between  $n$  abilities would be determined by numerous 0, 1st, 2d, 3d and higher order factors. In any  $n$  variable problem, one would in general expect all types from A to FF to occur, though due to the masking effects of sampling errors it would be difficult to point out with assurance some of these types.

Our conclusion, then, regarding the effects of group factors on tetrads would be as follows: If a two-factorist postulates certain group factors in a given problem the resulting  $T$ 's must behave rigorously (within sampling errors) according to the expected types as shown in Table II. If they do not,

TABLE II  
THE EFFECTS ON THE THREE TETRAD DIFFERENCES OF 1ST, 2D AND 3D ORDER GROUP FACTORS

		Full expectation			Approximate expectation			Total	
		$T'_{1344}$	$T''_{1344}$	$T'''_{1344}$	Type	$T'_{1344}$	$T''_{1344}$	$T'''_{1344}$	Type
<b>Section i. Patterns involving 1st order factors</b>									
. One 1st order factor in:									
a.	(12); (34)	+	+	o	A	+	+	o	A
b.	(13); (24)	-	o	-	B	-	o	-	B
c.	(14); (23)	o	-	o	C	-	o	-	C
. Two 1st order factors in:									
a.	(12); (34)	+	+	+	AA	+	+	+	AA
b.	(13); (24)	-	o	-	AB	-	o	-	BB
c.	(14); (23)	o	-	o	CC	-	o	-	CC
d.	(12); (13); (24)	+	+	+	o?	+	+	+	o?
e.	(34); (13); (24)	-	o	-	AB	o?	-	-	D
f.	(12); (14); (23)	+	o	-	AC	+	o?	-	E
g.	(34); (14); (23)	o	-	o	BC	-	o?	-	F
h.	(13); (14); (23)	-	o	-	-	-	-	-	4
i.	(24); (14); (23)	o	-	o	-	-	-	-	4
. Three 1st order factors in:									
a.	(12); (34); (13); (24)	+	+	+	AAB	+	+	+	G
b.	(12); (34); (14); (23)	-	o	-	AAC	+	+	-	H
c.	(13); (24); (12); (23)	+	o	-	BBA	-	o	-	I
d.	(13); (24); (14); (23)	+	o	-	BBC	-	o	-	J
e.	(14); (23); (12); (24)	-	o	-	CCA	+	-	-	K
f.	(14); (23); (13); (24)	o	-	o	CCB	-	-	-	L
g.	(12); (13); (14); (23); (24); (13); (14); (34)	o	-	o	ABC	o?	o?	o?	M
h.	(12); (24); (13); (24); (23); (34); (24); (14); (34)	-	o	-	-	-	-	-	8
. Four 1st order factors in:									
a.	(12); (34); (13); (24)	+	+	+	ABB	o?	+	+	DD
b.	(12); (34); (14); (23)	-	o	-	AAC	+	o?	-	EE
c.	(13); (24); (14); (23)	+	o	-	BBCC	-	-	-	FF
d.	(12); (34); (13); (14); (12); (34); (13); (23); (12); (34)	+	+	+	-	-	-	-	o?
e.	(24); (14); (12); (14); (13); (24); (23); (12); (23); (13); (24)	-	o	-	AABC	+	+	o?	A
f.	(13); (24); (12); (14); (13); (24); (23); (12); (23); (13); (24)	o	-	o	BBAC	-	o?	-	B
g.	(14); (23); (12); (13); (14); (23); (24); (12); (24); (14); (23)	o	-	o	CCAB	o?	-	-	C
h.	(34); (13); (14); (23); (34); (24); (23); (24); (23); (24); (23)	o	-	o	-	-	-	-	4

TABLE II (Continued)

	Full expectation			Approximate expectation			Total	
	$T'_{1234}$	$T''_{1234}$	$T'''_{1234}$	Type	$T'_{1234}$	$T''_{1234}$	$T'''_{1234}$	
V. Five 1st order factors in:								
<i>a.</i> (12) (34); (13) (24); (14); (12) (34); (13) (24); (23) . . .	+	+	-o	+++-	o?	+	+	D
<i>b.</i> (12) (34); (14); (23); (13); (24) . . .	+	+	-o	+++-	AABC	o?	+	2
<i>c.</i> (13) (24); (14); (23); (12); (24); (14); (23) . . .	+	+	-o	+++-	AACB	-	-	2
VI. Six 1st order factors:								
(12) (13) (14); (23) (24); (13) (24); (12) (34); (13) (24); (12) . . .	+	-o	-o	++-o+-o	BBCCA	-	-o?	F
					ABCABC	o?	-o?	M
						o?	o?	1

## Section iii. Patterns involving 2d order factors

N.B. To conserve space, only the approximate type will be given.

## VII. One 2d order factor in:

(123); (124); (134); (234) . . .

## VIII. Two 2d order factors in:

(123) (124); (123) (134); (123) (234); (124) (134); (124) (234) . . .

## IX. Three 2d order factors in:

(123) (124) (134); (123) (124) (234); (123) (134) (234) . . .

## X. Four 2d order factors in:

(123) (124) (134) (234) . . .

## Section iii. Patterns involving 3d order factors

## XI. One 3d order factor in:

(1234) . . .

## Section iv. Patterns involving 1st and 2d order factors

Those in section i in combination with those in section ii, there resulting 15 times the frequency of each of the approximate types shown in section i.

## Section v. Patterns involving 1st and 3d order factors

Those of section i in combination with those in section iii, there resulting 1 times the frequency of each of the approximate types shown in section i.

## Section vi. Patterns involving 2d and 3d order factors

Those of section ii in combination with those in section iii, there resulting 1 times the frequency of each of the approximate types shown in section i.

## Section vii. Patterns involving 1st, 2d and 3d order factors

Those of section vi in combination with those in section i, there resulting 15 times the frequency of each of the approximate types shown in section i.

TABLE II (Continued)  
SUMMARY TABLE

Patterns involving factors of:	Total number of each approximate type												All types							
	A	B	C	D	E	F	G	H	I	J	K	L	M	AA	BB	CC	DD	EE	FF	
1st order.....	6	6	6	6	6	6	6	2	2	2	2	2	9	1	1	1	1	1	63	
2d order.....	6	6	6	6	6	6	6	6	6	6	6	6	15	1	1	1	1	1	215	
3d order.....	90	90	90	90	90	90	90	30	30	30	30	30	135	15	15	15	15	15	945	
1st and 2d order.....	6	6	6	6	6	6	6	2	2	2	2	2	2	9	1	1	1	1	1	63
1st and 3d order.....	90	90	90	90	90	90	90	30	30	30	30	30	135	15	15	15	15	15	945	
2d and 3d order.....	192	192	192	192	192	192	192	64	64	64	64	64	319	32	32	32	32	32	2,047	
All classes.....	192	192	192	192	192	192	192	192	192	192	192	192	319	32	32	32	32	32	2,047	

then his particular factor pattern may be considered untenable. Even, however, if they do, this would not constitute *proof* of his modified two factor pattern, for many patterns of multiple factors without a *g* could have led to the very same types. But on the theory of a multiplicity of factors, one would not expect a few clear-cut types, but virtually the whole gamut of types. If such a variety is recovered, it would *prove* the operation of group factors, though it would not enable one to determine the number or weights. If one is a two-factorist one may *choose* the minimal number, including a monarchic *g*; if one has faith in a few oligarchic faculties, one may choose the minimal number with or without a *g*; if one believes in the operation of a wealth of factors, one would discern no need to choose any particular multiple-factor pattern but simply proclaim that whatever the correct pattern may be, it doubtless consists in numerous factors of different orders.

### III. THE STATISTICAL DETERMINATION OF GROUP FACTORS

#### *A. The Problem of Sampling Errors*

The above analysis has concerned itself with the approximate behavior of 'true' tetrads under different postulations regarding group factors. But in actual measurement one deals with limited samples, which in consequence give tetrads unsystematically deviating from the true values. To reduce this masking effect of sampling errors, in our later analyses we shall deal with samples containing at least 100 cases and often many more. Still, this does not rid one entirely of unreliability, so we must make some decision as to whether a given magnitude plus or minus zero is a true deviation and not an error. In order to keep our methods as similar to those of the two-factorists as possible we shall adopt the rough rule given by Spearman: "My co-workers and myself hold that the burden of proof of a group factor lies with him who asserts its existence, and the rule of our laboratory is that, for complete statistical proof, a result ought not to be much less than five times the probable error. This agrees well enough

with the rule of the wise Udny Yule, who says that a result, in order to be certainly significant, should be at least three times the standard error (and therefore about four and a half times the probable error). Results only about three times the probable error we take to be suggestive . . ." (11, p. 566). Thus if an experimenter working with  $n$  variables postulates a two factor determination plus *one* first order group factor, say, between  $x_1$  and  $x_2$ , then *all tetrads of the form*  $t_{12ij}$  ( $i, j = 3, 4, 5 \dots, n$ ,  $i$  and  $j$  not having the same value in the same tetrad) *must be about four and a half times the probable error*. But if some of these tetrad are *not*, then the group factor postulated has not been *proved* consistent with the data. A rejoinder may be made, however, that if the average of tetrads of form  $t_{12ij}$  is four and a half times the probable error then those less than the required value may be themselves chance fluctuations in the direction of zero. While there is a good rationale behind such a rejoinder, still, since the burden of the proof has resided in him who asserted the existence of the group factor, we must conclude that the asserter has not rigorously *proved* consistency with the postulated modified two factor pattern unless the majority of the tetrads expected to be significantly different from zero are actually shown to be so.

We shall see that in the attempts of two-factorists to explain away too large tetrads, they have not themselves followed the rule which they have set up. Among the many tetrads thrown out, beside those that really fit the rule there are many that should, but obviously do not. This fact enshrouds their analysis with inconclusiveness. The truth is that the purification technique and the rule which serves it represent a method quite inadequate to cope with this complex problem.

*An adequate proof that a postulated factor pattern is consistent with the observed tetrads is one which ascertains the coefficients (or weights) of the factors in each variable, reconstructs theoretical r's from these coefficients by means of Garnett's formula and then shows that these theoretical coefficients fit the observed*

ones within the requirements of sampling. If the theoretical  $r$ 's fit the observed, then the theoretical tetrad differences fit the observed, and consistency has been proved. Such thorough-going proofs have never, in the writer's knowledge, been made by two-factorists. Kelley makes just such a test of his theoretical patterns (7, p. 105ff.), noting that the observed correlation coefficients deviate from the theoretical by amounts expected from sampling errors. As part of his method, Thurstone makes a similar test for statistical consistency (17, p. 422).

#### IV. THE EVIDENCE REGARDING THE EXISTENCE OF GROUP FACTORS

In the following analyses we shall turn our attention primarily to the work of two-factorists, who have attempted by the postulation of the supplementary group factors listed earlier in the paper to pull into line with their theory the numerous instances in which the tetrad difference criterion has not been satisfied. We shall consider only those researches studied in the previous paper, to which the reader is referred for information as to the original sources of the data. We shall pass over Bonser's work, for no attempt has been made to explain away the too large tetrads manifest in it. We address ourselves first to the research of Spearman himself.

*Spearman's Study.*—In this research on 2,599 individuals, Spearman secured measures as follows (10, p. 153):

$x_1$ = Completions, selective	$x_5$ = Instructions
$x_2$ = Analogies, inventive	$x_6$ = Passages, inventive
$x_3$ = Completions, inventive	$x_7$ = Passages, selective
$x_4$ = Analogies, selective	

The distribution of tetrads showed many too large tetrads (10, 18), which Spearman attempted to explain away by postulating the following pattern:

$$\begin{array}{ll}
 x_1 = f(g, a, s_1) & x_5 = f(g, s_5) \\
 x_2 = f(g, b, s_2) & x_6 = f(g, c, s_6) \\
 x_3 = f(g, a, s_3) & x_7 = f(g, c, s_7) \\
 x_4 = f(g, b, s_4) &
 \end{array}$$

in which  $g$  and the  $s$ 's are the usual two factors,  $a$ ,  $b$  and  $c$  are first order group factors linking (13), (24) and (67), respectively. The 'sub-theory' presented to defend this was that the group factors were generated by 'similarity of form' of the tests. (To be psychologically consistent one would as lief anticipate the presence of two second order group factors, 'inventiveness' and 'selectiveness.') Spearman thus threw out about 50 percent of the tetrads—those involving  $r_{13}$ ,  $r_{24}$ ,  $r_{67}$ . Among these, however, were many which do not conform to his theory of overlap. Observe those in Table III.

TABLE III  
P.E. IS OF ORDER .007

T	Ex- pected type	Observed			In- const.	T	Ex- pected type	Observed			In- const.
		T'	T''	T'''				T''	T'''		
T <sub>1345</sub>	A	.036	.018	-.018	Im	T <sub>1367</sub>	A	.010	.012	.002	Im
T <sub>1246</sub>	C	-.016	-.032	-.016	Im	T <sub>1467</sub>	A	.002	.012	.010	Im
T <sub>2345</sub>	B	-.044	-.022	.022	Im	T <sub>1667</sub>	A	-.003	.010	.013	Im
T <sub>2468</sub>	A	.015	.027	.012	Im	T <sub>2367</sub>	A	.005	.012	.007	Im
T <sub>2467</sub>	A	.017	.036	.019	Im	T <sub>3667</sub>	A	.011	.016	.005	Im
T <sub>3467</sub>	A	.010	.012	.002	Im	T <sub>4667</sub>	A	.008	.010	.002	Im

The table is self-explanatory, giving the  $T$  under consideration, the expected type, the actually calculated (observed) values in each tetrad of the triplet-set, namely,  $T'$ ,  $T''$ ,  $T'''$ , and finally a notation regarding the nature of the inconsistency of the observed with the expected values. For instance, note the triplet-set,  $T_{3467}$ , supposed to overlap in (67). The expected result is type A, namely  $T' = T''$ , and equal to a value significantly different from zero, but actually, the observed values are some of the smallest in the table, being hardly greater than one P.E. Thus,  $T_{3467}$  suffers in being of insufficient magnitude (Im), and is not even suggestive of group factors by Spearman's rule. The tetrads in Table III are about half of those thrown out by Spearman as being too large. The values they take suggest that a more complicated multiple factor pattern would better fit the data.

than the modified two factor pattern as postulated by Spearman.

*Davey's Study.*—Oral and pictorial tests were given 243 school children, and the intercorrelations were analysed for the two factors (2). The measures were:

<i>Oral</i>	<i>Pictorial</i>
$x_1$ = Opposites	$x_9$ = Classification
$x_2$ = Synonyms	$x_{10}$ = Analogies
$x_3$ = Classification	$x_{11}$ = Sequence
$x_4$ = Questions	$x_{12}$ = Completions
$x_5$ = Completions	$x_{13}$ = Questions
$x_6$ = Analogies	$x_{14}$ = Enumeration
$x_7$ = Inference	
$x_8$ = Likliness	

But the criterion was not satisfied, there being too many too large tetrads (2, 18). After an examination of these tetrads, Davey finally concluded that "a group factor is common to the first four oral tests, but does not extend in any marked degree to the oral tests as a whole. There is no such evidence, however, for the presence of a similar group factor among the pictorial tests" (p. 40), and finally concludes "that this investigation has established the fact that a verbal mental test measures the same general factor 'g' as does a test similar in form but non-verbal in material."

Translating Davey's postulations into a factor pattern, we note that she enounces the following:

$$\begin{array}{lll}
 x_1 = f(g, a, s_1) & x_8 = f(g, s_6) & x_{11} = f(g, s_{11}) \\
 x_2 = f(g, a, s_2) & x_7 = f(g, s_7) & x_{12} = f(g, s_{12}) \\
 x_3 = f(g, a, s_3) & x_8 = f(g, s_8) & x_{13} = f(g, s_{13}) \\
 x_4 = f(g, a, s_4) & x_9 = f(g, s_9) & x_{14} = f(g, s_{14}) \\
 x_5 = f(g, s_5) & x_{10} = f(g, s_{10}) &
 \end{array}$$

where the  $a$  is the group factor postulated. If this were true, then all  $T$ 's of the form  $T_{ijkl}$  ( $i, j = 1, 2, 3, 4; k, l = 5, 6, \dots, 14$ ; the same variable number not occurring twice in the same tetrad), should be of type A. As the writer has not calculated

all of the tetrads, he can give in Table IV, only those calculated in the *T*-scheme, previously described (18, section IV).

TABLE IV  
P. E. IS OF ORDER .019

T	Ex- pected type	Observed			In- const.	T	Ex- pected type	Observed			In- const.
		T'	T''	T'''				T'	T''	T'''	
T <sub>1256</sub>	A	.00	.02	.02	Im	T <sub>3458</sub>	A	.03	-.01	-.04	Im
T <sub>1278</sub>	A	-.03	-.01	.02	Im	T <sub>3478</sub>	A	-.02	.01	.03	Im
T <sub>12910</sub>	A	.03	.05	.02	Im	T <sub>34910</sub>	A	.14	.12	-.02	None
T <sub>121112</sub>	A	.00	.03	.03	Im	T <sub>341112</sub>	A	.06	.04	-.02	Im
T <sub>121314</sub>	A	.07	.02	-.05	?	T <sub>341314</sub>	A	.10	.06	-.04	?

There is only one *T* in which the observed values are consistent with expectation, most of the others showing insufficient magnitudes, or unintelligible values. This would doubtless be true of the remaining tetrads discarded by Davey. The present writer has also made a cumulative distribution of the tetrads of the *T*-scheme involving variables 5 to 14, which according to Davey's factor pattern should vanish, and they do not form a chance distribution as Davey contended. Thus, the proper conclusion to be drawn from this study is that the theory of two factors with the modifying group factor as postulated by Davey has not been proved consistent with the data, and that a more complex multiple factor pattern is demanded.

The reader should recall that in his own study, Spearman advanced 'similarity of form' of the tests as the cause of overlap between tests (though we found such an hypothesis inadequate). But in Davey's study there was not the slightest evidence of such a phenomenon. Note that completions, analogies, questions and classification occurred both as oral and as pictorial tests, but Davey was unable to discern such similarity as generative of a group factor. She was, in fact, unable to advance any definite *a posteriori* explanation of her 'group factor' between the first four oral abilities, dismissing the matter briefly and obscurely: "The source of

this group factor in the first half of the oral tests . . . must therefore be attributed to their content, that is, either to the fundaments, or to the similarity of the relations educed in the solution of the tests (p. 41)."

*Stephenson's Study.*—A more comprehensive research on verbal and non-verbal abilities was that of Stephenson, assistant to Spearman. The subjects were 1,037 school girls, who were given the following tests:

<i>Non-Verbal</i>	<i>Verbal</i>
$x_1$ = Alphabet construction	$x_9$ = Synonyms
$x_2$ = Code	$x_{10}$ = Sentence completion
$x_3$ = Fitting shapes	$x_{11}$ = Classification
$x_4$ = Picture completion	$x_{12}$ = Interchanged words
$x_5$ = Analogies, form	$x_{13}$ = Opposite
$x_6$ = Counting cubes	$x_{14}$ = Analogies
$x_7$ = XO completion	$x_{15}$ = Always has
$x_8$ = Overlapping shapes	$x_{16}$ = Directions

The present writer considers this experiment to be one of the most conclusive demonstrations of the inadequacy of two factors. It shows the need to consider mental abilities to be multiple factorially determined, for the tetrad difference criterion seemed to resist being satisfied despite the arduous efforts of Stephenson to manipulate the data to satisfy it.

The first step in the analysis was to see if the tetrads of the non-verbal tests *alone* satisfied the criterion (14), and the results showed that they definitely did not do so (14, 18). Stephenson's attempts to warp these data into consistency with the two factor theory were as follows: he rescaled each variable into the 'normal' form, the reasoning behind such a process being made not clear, and in the process mysteriously dropped out variables  $x_6$  and  $x_7$ . The tetrads of the six remaining non-verbal abilities still did not satisfy the criterion. Then, Stephenson noted that by throwing out  $r_{28}$ , the tetrads involving the remaining  $r$ 's tended to satisfy the criterion. His *a posteriori* postulation of a group factor between  $x_2$  and  $x_8$  was justified by him through a claim that they both

involved 'speed preference.' In this connection, one immediately wonders whether some of the other measures did not *also* involve 'speed preference,' and if they did, why they did not manifest in the tetrads the presence of the group factor. But as we have seen, the purification technique does not require such psychological consistency, so this matter was not considered.

The next step was to see if the tetrads of the verbal sub-tests *alone* satisfied the criterion. With these, the criterion was even less satisfied than with the non-verbal tests (15, 18). Every effort at purification was fruitless:  $x_9$  was thrown out as being a 'shock-absorber,' the test-scores were transmuted into 'normal' distributions; group factors generated by 'similarity of relations,' 'idiosyncrasies,' 'speed preference,' 'propinquity,' 'group testing,' 'school and class' were tentatively postulated; yet all such manipulations left the criterion still grossly not satisfied. Stephenson was therefore forced to conclude that these "sources of error so far brought forward appear inadequate to explain the whole of the excess error in the tetrads" (p. 267).

Finally, the problem was considered whether verbal and non-verbal abilities together might be thought of as determined by  $g$  and the  $s$ 's, depending upon whether or not the tetrads involving the 'cross' correlations between verbal and non-verbal tests satisfied the criterion (16, 18). Since, as we have seen, there was no evidence indicating that the verbal abilities were determined by a  $g$ , it would seem unlikely that these with non-verbal abilities would miraculously manifest the existence of a  $g$ . Some hope could be held out, however, if one supposed the verbal abilities all to be a function of a second factor, namely, 'verbality.' This situation would account for the criterion not being satisfied by the verbal tests and would still save the day for  $g$ . If this were true, though, one would expect that tetrads involving correlations *between* verbal and non-verbal abilities but not containing any inter-correlations between the verbal tests themselves would satisfy the criterion. Such tetrads were studied by Stephenson, and

were divided into two classes. The first was of the type  $t_{nvn}$  (where  $n$  stands for different non-verbal tests,  $v$  for each verbal test). Stephenson examined two samples of these, apparently 324 in total, and they showed many too large tetrads. Even throwing out two-thirds of these on the *a posteriori* grounds of 'similarity of form,' 'similarity of relations,' 'speed preference,' with all the inconsistency which this process entails, the criterion was still not satisfied, though the situation was somewhat improved. The second type of tetrad was of the form  $t_{vvvn}$ . A total of 180 showed again many too large tetrads, and the criterion was still not satisfied by 'purifying' these. The excess divergence above sampling errors of the  $t_{nvn}$  and the  $t_{vvvn}$  after the laborious purifying, was finally 'explained' as probably the result of the 'summation of a few slight errors due to effects such as age, calculation mistakes, etc.' Such a method is, of course, a very unscientific 'proof' of the consistency of the two factor theory with the observed correlation. *In the final analysis, the evidence for the theory that the verbal and non-verbal abilities were determined by a  $g$  was based upon the behavior of only a few hundred tetrads which did not themselves exactly satisfy the criterion, and which were a highly selected sample drawn from many hundreds of tetrads.*

Stephenson's final argument for the postulation of a group factor, 'verbality,' among all the verbal tests, rested upon what was called the *specific* intercorrelations between the verbal tests, with  $g$  partialled out. The tetrads formed from these should vanish if such a group factor existed, for it would now constitute the only general factor left among the verbal tests. But again the criterion was not satisfied, and remained not satisfied even after the tetrads containing  $r$ 's supposedly involving specificality due to 'similarity of relations' were eliminated. The remaining excess divergence from zero after 'purification' was again ascribed to summation of other errors.

Our own interpretation of Stephenson's work is that it excellently demonstrates the effects of multiple factors as determiners of abilities. Since he dealt with such a large  $N$ ,

the result was that the tetrad differences approximated closely their true values. These values *obviously* did not satisfy the criterion and could not be made to do so. The observed *T*'s took values running through almost the whole gamut of types shown in Table II, and indicated clearly the operation of numerous factors of different orders.

*Holzinger's Analysis of Kelley's Study.*—In his *Crossroads in the Mind of Man*, Kelley gave 140 seventh-grade children nine tests (7), as follows:

$x_1$ = Reading speed	$x_6$ = Memory for numbers
$x_2$ = Reading power	$x_7$ = Memory for meaningful symbols
$x_3$ = Arithmetic speed	$x_8$ = Memory for meaningless symbols
$x_4$ = Arithmetic power	
$x_5$ = Memory for words	$x_9$ = Space power

The factors or faculties which he proposed as the determiners of these abilities are represented in the following factor pattern (leaving out factors with negligible weights):

$$\begin{array}{lll} x_1 = f(a, b, f, s_1) & x_4 = f(a, c, e, f, s_4) & x_7 = f(a, d, e, s_7) \\ x_2 = f(a, b, d, s_2) & x_5 = f(a, b, d, s_5) & x_8 = f(a, d, e, s_8) \\ x_3 = f(a, c, f, s_3) & x_6 = f(a, c, d, s_6) & x_9 = f(a, c, e, f, s_9) \end{array}$$

where *a* is a general factor (race, sex, heterogeneity), *b* a verbal factor, *c* a number factor, *d* a memory factor, *e* a spatial factor, *f* a speed factor, and the *s*'s are the specific (chance and not chance) factors. By his iteration method, Kelley found satisfactory weights for each factor in the ability in question. Putting the weights in Garnett's formula, the resulting theoretical *r*'s fitted the observed *r*'s within the expectations of sampling, thus proving the statistical consistency of his theory with observation (7, Tables 13, 14).

Claiming that Kelley's factor pattern was 'needlessly complex,' Holzinger attempted to reanalyse the data by the usual tetrad technique (5). This reanalysis was, in fact, not utterly necessary, for Kelley's pattern had a general factor (though he refused to call it *g*), and the group factors he

postulated were not fundamentally different from those admitted by Spearman. Furthermore, Holzinger's reanalysis, being of the usual *a posteriori* two factor type, led to the usual psychological and statistical inconsistencies. Holzinger's proposed pattern was as follows:

$$\begin{array}{lll} x_1 = f(a, b, d, s_1) & x_4 = f(a, c, s_4) & x_7 = f(a, s_7) \\ x_2 = f(a, b, s_2) & x_5 = f(a, e, s_5) & x_8 = f(a, s_8) \\ x_3 = f(a, c, d, s_3) & x_6 = f(a, e, s_6) & x_9 = (\text{left out!}) \end{array}$$

where *a* is *g*, *b* is a 'reading factor' (verbality?), *c* is an arithmetic, *d* is a speed, and *e* is a memory factor, the *s*'s specifics. Holzinger stated that all that was claimed for his pattern was that "it is a reasonable one, and . . . yields tetrad differences in good accord with those obtained from the data." The present writer believes that neither of these claims is valid.

In the first place, the pattern is psychologically not reasonable or consistent, for why should the memory factor, *e*, not run through all of the memory tests,  $x_5$ ,  $x_6$ ,  $x_7$  and  $x_8$ , instead of through just  $x_5$  and  $x_6$ , as Holzinger's pattern stipulates? Why should there not be a space factor (such as Kelley's *e*) running through  $x_7$  and  $x_8$ , which deal with spatial symbols? Why should the arithmetic factor be absent in  $x_6$  which deals with numbers? Why should the verbal factor be absent from  $x_5$  which deals with words? Kelley's is much more reasonable since it does not contain so many of these psychological inconsistencies.

In the second place, Holzinger does not *prove* that his pattern yields tetrads in good accord with the data, while, as we have seen, Kelley does prove consistency. Let us observe the expected form of some of the *T*'s according to Holzinger's pattern, and then see what the actual values of these tetrads are. About one-fifth of the tetrads, drawn from Holzinger's own computations (5, p. 93), are shown in Table V.

These take values either of insufficient magnitude to support the theory, or when significant, show values not in conformity

TABLE V  
P.E. IS OF ORDER .03

T	Ex- pected type	Observed			In- const.	T	Ex- pected type	Observed			In- const.
		T'	T''	T'''				T'	T''	T'''	
T <sub>1267</sub>	A	.147	.085	-.062	?	T <sub>4567</sub>	C	-.100	-.084	.016	?
T <sub>1456</sub>	A	-.014	.008	.022	Im	T <sub>4568</sub>	C	-.060	-.129	-.069	?
T <sub>2358</sub>	A	-.110	-.053	.057	?	T <sub>1235</sub>	D	-.048	.038	.086	Im
T <sub>2468</sub>	A	-.043	.003	.046	Im	T <sub>1237</sub>	D	-.066	.030	.096	Im
T <sub>2568</sub>	C	.017	-.023	-.040	Im	T <sub>1345</sub>	E	.034	-.047	-.081	Im
T <sub>3567</sub>	C	-.125	.012	.137	?	T <sub>1234</sub>	G	.253	.272	.019	?
T <sub>3588</sub>	C	-.085	-.020	.065	Im						

with the theory. We must conclude, therefore, that Holzinger has not proved his pattern consistent with the data, which in reality looks as if multiple factors of many different orders are at work.

*Garrett's Study.*—To ascertain whether memory-learning tests were determined by the same general factor as that determining intelligence, Garrett gave 158 men students the following tests (3):

$x_1$ = Digit-span (visual)	$x_6$ = Digit-symbol
$x_2$ = Digit-span (auditory)	$x_7$ = Turkish-English vocabulary
$x_3$ = Paired assoc. (vis.)	$x_8$ = Code learning
$x_4$ = Paired assoc. (aud.)	$x_9$ = Thorndike Intelligence
$x_5$ = Logical memory	

But the tetrad differences formed from the intercorrelations among the memory-learning tests far from satisfied the criterion (3, 18), hence not even a single general factor among these abilities could be postulated. Using the same *a posteriori* methods of the two-factorists, Garrett now examined the tetrads and found that the too large ones involved  $r_{12}$ ,  $r_{34}$ ,  $r_{68}$ , so he hypothesized three 1st order group factors to cover these, and then by excluding tetrads involving them, observed, of course, that the remaining ones satisfied the criterion. Thus he concluded that, except for the three group factors occasioned by 'similarity of content,' the memory-learning tests were determined by a general factor.

Garrett sought now to ascertain whether this general factor was  $g$  or a general memory factor. He partialled out from all memory-learning intercorrelations the intelligence measure which he assumed to be a pure measure of  $g$  and noted a residual average correlation of .11 left among the memory-learning intercorrelations. This he concluded (tentatively, to be sure) to indicate the presence of a general memory factor.

But these results are inconclusive. In the first place, if one is to take seriously 'similarity of content' as a criterion of the presence of a group factor, then one must consistently apply it to all the variables involved. Such consistency would require, for Garrett's variables, a visual factor, an auditory factor, a digit-span factor, a paired associates factor, a verbal factor, a spatial factor, a general memory factor, and possibly others.

Secondly, the three group factors postulated by Garrett are not statistically consistent with the observed data. On Garrett's postulation (excluding  $x_9$ ), 42 of the total 70  $T$ 's possible should be of types A, B, C, or AA if Garrett's pattern is correct. As a matter of fact, however, 20 of the 42 are clearly not so, as Table VI shows.

TABLE VI  
P.E. IS OF ORDER .025

T	Ex- pected type	Observed			In- const.	T	Ex- pected type	Observed			In- const.
		T'	T''	T'''				T'	T''	T'''	
T <sub>1246</sub>	A	.07	.07	-.01	Im	T <sub>2468</sub>	A	-.01	-.01	.00	Im
T <sub>1256</sub>	A	.01	.03	-.02	Im	T <sub>2467</sub>	A	.04	.13	.09	?
T <sub>1258</sub>	A	.04	.05	.01	Im	T <sub>1368</sub>	A	-.02	-.01	.01	Im
T <sub>1245</sub>	C	.01	-.01	-.02	Im	T <sub>1468</sub>	A	-.03	-.01	.02	Im
T <sub>1348</sub>	C	.01	-.05	-.06	Im	T <sub>1568</sub>	A	.01	.02	.01	Im
T <sub>2345</sub>	C	-.03	.06	.09	Im	T <sub>1768</sub>	A	.05	.08	.03	Im
T <sub>2346</sub>	C	-.03	-.07	-.04	Im	T <sub>2268</sub>	A	-.06	-.08	-.02	Im
T <sub>2347</sub>	C	-.04	-.10	-.06	Im	T <sub>2468</sub>	A	.02	.02	.00	Im
T <sub>2456</sub>	A	-.02	-.05	-.03	Im	T <sub>2568</sub>	A	-.08	-.07	.01	Im
T <sub>2457</sub>	A	.00	.05	.05	Im	T <sub>2768</sub>	A	.03	.03	.00	Im

In practically no instance above is the expected  $T$  recovered, using Spearman's rule as a criterion of proof of a significant

difference from zero. The conclusion we draw from Garrett's study is that none of the group factors postulated by him have been proved consistent with the observed correlations, and that, the very unsystematic values taken by the tetrads indicate rather a complex determination by multiple factors.

*Anastasi's Study.*—Dealing exclusively with tests of immediate memory for visually presented material, Anastasi attempted to discover, as did Garrett, whether a general memory factor could be postulated. The tests finally analysed were:

- $x_1$  = Paired assoc.: word-word
- $x_2$  = Paired assoc.: picture-number
- $x_3$  = Paired assoc.: form-number
- $x_4$  = Paired assoc.: color-word
- $x_5$  = Retained members: (words)
- $x_6$  = Syllable recognition

The tetrad differences formed from the intercorrelations among these tests failed to satisfy the criterion (1, 18). There was no clear evidence either of a factor common to *all* the paired associates tests, as Garrett had postulated, or of a verbal factor among the tests involving words. Anastasi finally decided that there was a group factor between (23). This is psychologically consistent, since these were the only two tests involving numbers. She postulated also a 1st order factor between (45). But this does not make psychological sense, for there seems *a priori* to be no *unique* association between these two tests. But these group factors are not statistically consistent with the tetrads, for many of the tetrads involving  $r_{23}$  and  $r_{45}$  which should have shown the characteristic form of types A, B, or C clearly *did* satisfy the tetrad difference criterion, as an examination of her Table VII (1), containing all the tetrad differences, will divulge. The values of the tetrads took in general such a complex series of values that the only consistent theory of factors to be postulated would be a complex multiple factor theory.

*Schneck's Study.*—Using 210 students of a similar sample to that of Anastasi's, Schneck attempted to discover whether verbal and numerical abilities were determined by the same common factor. His tests were as follows:

<i>Verbal</i>	<i>Numerical</i>
$x_1$ = Vocabulary	$x_6$ = Arithmetic reasoning
$x_2$ = Opposites	$x_7$ = Number completion
$x_3$ = Analogies	$x_8$ = Equation relations
$x_4$ = Sentence completion	$x_9$ = Mental multiplication
$x_5$ = Disarranged sentences	

The tetrad difference criterion was grossly not satisfied (9, 18). Schneck concluded from his tetrad analysis that the verbal abilities had a common factor running through them and so did the numerical abilities, but that these two factors were not the same. Schneck's method of analysis was to calculate the tetrad differences among the verbal abilities alone and then among the numerical abilities alone. He claimed that in each separate analysis the criterion was satisfied. But in his analysis of the verbal abilities he threw out all tetrads involving  $r_{12}$ , asserting that the vocabulary and opposites tests were too similar. This is our old friend, the purification technique, so we must look at the discarded tetrads to see if they support the contention of a special linkage in (12). They follow:

TABLE VII  
P.E. IS OF ORDER .023

<i>T</i>	Expected type	Observed			Inconst.
		<i>T'</i>	<i>T''</i>	<i>T'''</i>	
$T_{1234}$ .....	A	.033	.019	-.014	Im
$T_{1235}$ .....	A	.095	.053	-.042	?
$T_{1245}$ .....	A	.089	.064	-.025	?

The tetrads either take values of insufficient magnitude to prove the presence of a special linkage, or when significant the tetrads are not of clear-cut type A.

We must conclude that the special linkage has not been proved. Furthermore, though each of the tetrads *not* discarded by Schneck did not exceed three times the probable error, as a group these tetrads took values suspiciously large, there were too few of them, however, to form a distribution (as illustrated in the previous paper) and test for normality. The present writer considers that the thesis that the verbal abilities could be considered to be determined by one common factor, and the numerical abilities also by one factor, was not proved.

The attempt by Schneck to ascertain whether the postulated two group factors were the same general factor led to further ambiguities. He found that 'cross tetrads,' each involving two verbal and two numerical variables tended in general not to vanish. He thus properly concluded that a single general factor (*sans* group factors) did not run through these two abilities. To account for these results, Schneck now assumed that a general factor actually did run through all these abilities, but that either the numerical or the verbal abilities, or both of them, possessed a group factor *besides* the general factor (p. 39). He believed, that is, that one of the following three patterns was correct:

$$x_i = f(a, b, s_i) \quad \text{or} \quad x_i = f(a, b, s_j) \quad \text{or} \quad x_i = f(a, s_i) \\ x_j = f(a, c, s_j) \quad \text{or} \quad x_j = f(a, s_j) \quad \text{or} \quad x_j = f(a, c, s_j)$$

where  $i = 2, 3, 4, 5$ , and  $j = 6, 7, 8, 9$ ,  $a$  is the general factor ( $g?$ ),  $b$  is the group factor among the verbal abilities,  $c$  is the group factor among the numerical abilities. Note here that in each of these patterns, at least one of the two types of abilities is postulated as being determined by *two group factors*. For instance, in the first pattern all of the verbal abilities are supposed to be determined by  $a$  and  $b$ , two factors general relative to the verbal abilities. If this is true, then the tetrad difference criterion should not *necessarily* be satisfied, as Schneck asserted it was for these abilities. Rather, the criterion to be satisfied is Kelley's pentad criterion (7, p. 58) or his proposition 12 (7, p. 52). We must conclude that Schneck has therefore not proved

any of the patterns he suggests as consistent with the observed correlations.

The present writer sees no need to postulate (nor evidence of) several clear-cut factors in Schneck's abilities. The behavior of the intercorrelations and of the tetrads indicates a complex multiple factor determination, such as the following:

$x_i = f$  (a few 1st, 2d, but many 3d order verbal factors + a few 8th order verbal-numerical factors + many specifics)

$x_j = f$  (a few 1st, 2d, but many 3d order numerical factors + a few 8th order verbal-numerical factors + many specifics)

Such a pattern would result in a *tendency* for the tetrads involving the verbal and numerical abilities taken separately to satisfy the criterion, whereas the tetrads involving the cross correlations between the two types of abilities should show a marked tendency *not* to vanish. And these are the results which were actually recovered.

*Minnesota Mechanical Abilities Study.*—In the previous paper (18) we saw that the seven mechanical measures secured on 100 school boys rendered intercorrelations which when submitted to tetrad analysis clearly showed the criterion not satisfied. The Minnesota investigators properly concluded that no single general factor is manifest in these abilities, and that innumerable group factors must be postulated (8). The present writer has examined many of the triplet-set  $T$ 's, and sees no clear-cut evidence of a *few* group factors sufficient to explain the behavior of the tetrads, and concludes that here again a complex multiple factor pattern must be postulated.

The authors of the Minnesota study concluded that mechanical ability is a 'unique trait,' this on the basis of the low correlation between the mechanical ability test battery and other non-mechanical criteria (intelligence, agility, etc.). This must not be interpreted to mean that mechanical ability is itself a unitary trait different from everything else. The evidence shows, of course, that the mechanical abilities

entering into the battery are about as independent of each other as the whole battery is independent of other measures. The so-called mechanical ability consists simply of those abilities which, on the basis of their social utility, we chose to lump together and designate by the label 'mechanical ability.'

Spearman, it is to be recalled, has found it necessary to postulate a mechanical group factor (10, p. 229). Since we have observed, however, that *investigations concerned with cognitive abilities other than mechanical have satisfied the criterion no better than has this Minnesota study on mechanical abilities alone*, then there seems no good reason why we should set mechanical ability off to one side and demand for it the postulation of a group factor. The tetrads involving mechanical measures and those involving other mental capacities show excess divergence from zero over the amount expected from sampling errors, and the same array of complex values, and hence it is reasonable to postulate for both cases the operation of numerous multiple factors of different orders.

*The Study of Hartshorne and May.*—Finally, we turn to the so-called character traits. That there is no evidence of a single general factor running through the measures secured by Hartshorne and May (4) on approximately 190 children has been already noted (18). Are there any clear-cut group factors at work? The measures concerned were:

$x_1$  = Honesty       $x_3$  = Inhibition       $x_5$  = Moral knowledge  
 $x_2$  = Service       $x_4$  = Persistence

And here are the tetrad differences:

TABLE VIII  
P.E. IS OF ORDER .021

T	Observed			T	Observed			T	Observed		
	T'	T''	T'''		T'	T''	T'''		T'	T''	T'''
T <sub>1234</sub>	-.026	-.003	.023	T <sub>1245</sub>	-.046	-.017	.029	T <sub>2345</sub>	.030	-.041	-.071
T <sub>1235</sub>	-.077	-.028	.049	T <sub>1345</sub>	.095	.024	-.071				

None of these tetrads exceeds five times the P.E. but still the values they do take are too large to constitute chance deviation from zero. The tetrads do not fall into any such simple types as to indicate the existence of a few clear-cut factors. So we conclude here, again, that a few 'functional unities' are not at work in the determination of character, but that a multiplicity of factors of diverse orders are at work.

#### V. CONCLUSIONS

In the previous and present papers we discovered that in ten experimental researches in which correlations between abilities had been determined accurately, no evidence was obtained which indicated that the tetrad difference criterion was satisfied. We concluded therefore that no good reason exists which would cause us to entertain the hypothesis of a single general factor,  $g$ , running through all abilities, but we were forced to conclude, on the contrary, that numerous factors were doubtless at work. We were confronted by the fact, however, that two-factorists had considered that they had satisfactorily rationalized such discrepant results into consistency with their theory of  $g$  by asserting that a few clear-cut group factors (out of a possible list of forty odd) operate in each of these experiments in such a fashion as to cause some of the tetrad differences to be too large. To investigate this assertion, we have made an intimate analysis of these tetrad differences and have observed in practically no instance that the too-large tetrads behaved in the systematic fashion required by these group factor postulations. We found no evidence of a monarchic  $g$  with a small retinue of functional unitary 'faculties.' In fact, the tetrads inevitably took such complex values as again to indicate that the most plausible hypothesis of determination was one postulating numerous factors of all degrees of overlap between mental variables.

It must not be understood that the writer asserts that the evidence *cannot* be warped into congruity with the theory of a general factor. All that he asserts is that such con-

gruity has never been *proved* by two-factorists. This lack of proof has been largely due to the inadequacy of simple tetrad technique, as used by two-factorists, to prove consistency of results with the theory. To prove such consistency, they must utilize some such technique as that suggested by Kelley or Thurstone, in which theoretical correlation coefficients are calculated on the basis of the modified two factor pattern postulated, and these shown to fit (within the errors of sampling) the actually determined coefficients. Furthermore, besides such statistical consistency, the group factors postulated should make psychological sense. For example, if a memory group factor is postulated and found to be statistically consistent with the observed correlations, one should ordinarily expect all abilities obviously mnemonic to reveal it. If they do not, then the postulated factor cannot invite serious consideration. Most of the analyses made by two-factorists have not, as we have seen, satisfied these requirements of statistical and psychological consistency.

Even though it may be ultimately shown that a relatively small number of general and group factors enjoy both statistical and psychological consistency with the observed facts of intercorrelation between mental abilities, this would not *require* of us that we accept such factors as the real determiners at work. There is still a third criterion of consistency which must be fulfilled. This is that such factors should be consonant with other facts of psychology and biology not determined in the particular intercorrelational investigations under consideration. Thus, such factors must be intelligible in terms of the existing evidence of genetics and the psychology of development. It is the writer's belief that the hypothesis of a few great factors or faculties as determiners of individual differences is *not* so consistent, but rather that the numerousness and independence of hereditary determiners, and of the materials of introspective association or objective conditioning should dispose us to the hypothesis that a large number of more or less independent factors

working in all degrees of overlap determine the intercorrelation between abilities.

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## THE PLEASURE-PAIN THEORY OF LEARNING

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According to the pleasure-pain theory of learning, organisms select those modes of behavior which are accompanied or followed by pleasure and eliminate those that are accompanied or followed by pain. This theory was originally proposed by Spencer and Bain as an explanation of the means by which animals become adapted to their environments, and it has been strongly defended as a scientific description of the formation of all kinds of habits. An animal finds himself in an unsatisfactory situation and goes through a series of random movements. The pleasurable movements are beneficial and the painful ones are injurious; and after many generations those movements that are pleasant and beneficial are acquired by a process of natural selection. In the development of the individual and also in the evolution of the race, both Spencer and Bain thought that there was a general tendency for organisms to select the pleasant and successful movements.

The pleasure-pain theory has made a strong appeal to the layman, and Thorndike's statement of the law of effect has had such an important influence on education that during the past generation the teachers colleges and normal schools have based their most approved classroom procedure on the earlier studies of cats, dogs, and chickens. The law of effect still remains one of the most intriguing devices in attempting to "bridge the gap between 'theory' and practice."

It appears that few things could be more important for the psychology of learning than a clear understanding of the evidence on which this theory is based, and in the present paper we shall bring together the experimental and observational data which have a direct bearing on the theory. After an examination of this evidence an attempt will be made to arrive at a general conclusion on the whole question. Priority

of authorship is an important consideration in the present topic, and we shall discuss first the contributions of Spencer, Bain, and Baldwin. Thorndike's views will be considered because so much of the recent discussion has centered around his theories and experiments.<sup>1</sup>

#### SECTION I. THE SPENCER-BAIN-BALDWIN THEORY OF LEARNING

After describing and criticizing the views of Spencer, Bain, and Baldwin it will not be necessary to discuss the theories and opinions of a number of later writers. These three writers also deserve special consideration because they describe the pleasure-pain theory of learning in a scholarly fashion, and they did not go to uncritical extremes in their arguments.

Herbert Spencer claimed that the relations between the pleasant and the beneficial and between the painful and the injurious are primary and were established by natural selection. "If we substitute for the word *Pleasure*," he wrote, "the equivalent phrase—a feeling which we seek to *bring into consciousness* and retain there, and if we substitute for the word *Pain* the equivalent phrase—a feeling which we seek to *get out of consciousness* and to keep out; we see at once that, if the states of consciousness which a creature endeavors to maintain are the *correlatives of injurious actions*, and if the states of consciousness which it endeavors to expel are the *correlatives of beneficial actions*, it must quickly disappear through persistence in the *injurious* and avoidance of the *beneficial*. In other words, those races of beings only can have survived in which, on the average, agreeable or desired feelings went along with activities conducive to the maintenance of life, while disagreeable and habitually-avoided feelings went along with activities directly or indirectly destructive of life; and there must ever have been, other things equal, the most numerous and long-continued survivals

<sup>1</sup> Some discussion of the origin of the views of Spencer and Bain may be found in H. R. Marshall, *Pain, pleasure, and æsthetics*, 1894, 183-8; and in B. I. Gilman, *Syllabus of lectures on the psychology of pain and pleasure*, *Amer. J. Psychol.*, 1893, 6, 53-60.

among races in which these adjustments of feelings to actions were the best, tending ever to bring about perfect adjustment."<sup>2</sup> The assumption that animals and men tend to *repeat* those acts that are *pleasant*, and to *avoid* those that are *painful*, calls for some kind of *physiological* explanation, and Spencer gave an explanation in purely physiological terms. He said that *pleasure* accompanies *increased nervous activity* and that *pain* accompanies *decreased nervous activity*. Some of the random movements of an animal produce an increased nervous activity which is accompanied by pleasure, and this condition reinforces the movements and causes them to be repeated again. Painful movements tend to be eliminated in a similar way because of the decreased nervous activity.

The best statement of Spencer's theory of learning is found in the following passage, which from the historical point of view deserves much more attention than it has received. "As nervous structures become more complex and more integrated, the network of their connexions becomes so close that every *special* muscular excitement is accompanied by some *general* muscular excitement. Along with the concentrated discharge to *particular muscles*, the ganglionic plexuses inevitably carry off a certain diffused discharge to *the muscles at large*; and this diffused discharge produces on them very variable results. Suppose now, that in putting out its head to seize prey scarcely within reach, a creature has repeatedly failed. Suppose that along with the group of motor actions approximately adapted to seize prey at this distance, the diffused discharge is, on some occasion, so distributed throughout the muscular system as to cause a *slight forward movement of the body*. Success will occur instead of failure; and after success will immediately come certain *pleasurable sensations with an accompanying large draught of nervous energy towards the organs employed in eating, etc.* That is to say, the *lines of nervous communication* through which the diffused discharge happened in this case to pass, have opened a *new way to certain*

<sup>2</sup> H. Spencer, *The principles of psychology*, I., 2d ed., 1870, 280. Italics mine. G. Allen, *A disciple of Spencer developed this theory further, in his Physiological æsthetics*, 1877, 5-29.

*wide channels of escape*; and, consequently, they have suddenly become lines through which a large quantity of molecular motion is drawn, and lines which are so rendered *more permeable* than before. On recurrence of the circumstances, *these muscular movements that were followed by success are likely to be repeated*: what was at first an *accidental combination* of motions will now be a *combination having considerable probability*. For when on such subsequent occasion the *visual impressions* have produced nascent tendencies to the acts approximately fitted to seize the object, and when through these there are nascently excited all the states, sensory and motor, which accompany capture, it must happen that among the links in the connected excitations there will be excitations of *those fibers and cells* through which, on the previous occasion, the diffused discharge brought about the actions that caused *success*. *The tendency for the diffused discharge to follow these lines will obviously be greater than before*; and the probability of a successfully modified action will therefore be greater than before. *Every repetition of it will make still more permeable the new channels*, and increase the probability of subsequent repetitions; until at length the nervous connexions become *organized*.<sup>3</sup> Spencer was especially concerned with what is beneficial or injurious to the organism, but it is interesting to note that his physiological theory of learning was formulated in 1870, and it is quite similar to, if not identical with, Thorndike's statements of the law of effect in 1908 and 1911.

Alexander Bain accepted the principal features of Spencer's theory, but gave a more subjective interpretation of animal learning. He said that states of pleasure are connected with an increase, and states of pain are connected with an abatement of some, or all, of the vital functions.<sup>4</sup> The muscles act spontaneously "previous to that cementing process which gives them a definite and purposed direction," but emotional excitement does not furnish the starting point. There is a tendency to put forth muscular power in the absence of any

<sup>3</sup> H. Spencer, *The principles of psychology*, 544-5. Italics mine.

<sup>4</sup> A. Bain, *The senses and the intellect*, 3d ed., 1874, 282-93. See also *Mind and body, the theories of their relation*, 1874, 59-75; and *The emotions and the will*, 3d ed., 1876, 313.

emotional wave whatever. Indeterminate spontaneity of muscular movements, or the spontaneous occurrence of central discharges, is a more primordial source than the determinate expression of special emotions. An emotional wave simultaneously imparts movements to a number of organs, but what is needed is an isolated or specific prompting in the first instance. Bain's statement of the law of effect is as follows. "We suppose movements *spontaneously* begun, and accidentally causing *pleasure*; we then assume that with the *pleasure* there will be an increase of vital energy, in which increase the fortunate movements will share, and thereby increase the pleasure. Or, on the other hand, we suppose the spontaneous movements to give *pain*, and assume that, with the *pain*, there will be a decrease of energy, extending to the movements that cause the *evil*, and thereby providing a remedy. A few repetitions of the fortuitous concurrence of pleasure and a certain movement, will lead to the forging of an acquired connection, under the law of Retentiveness or Contiguity, so that, at an after time, the *pleasure* or its *idea* shall evoke the proper movement at once."<sup>6</sup>

The relation between Bain's views and those of Spencer is described by Bain as follows. "My leading postulates—Spontaneity, the Continuing of an action that gives Pleasure, and the Contiguous growth of an accidental connexion,—are all involved in Mr. Spencer's explanation of the development of our activity. It would be strange to me if they were not. The spontaneous commencement is expressed by him as a diffused discharge of muscular energy. . . . This is the doctrine of Spontaneity in a very contracted shape; too contracted, in my judgement, for the requirements of the case. I have adverted to the inferiority of the diffused wave accompanying a central impulse, whether active or emotional, such as is here assumed. If another source of chance muscular movements can be assigned, and if that source presents advantages over the diffused discharge, we ought to include it in our hypothesis. . . . The second indispensable requisite

<sup>6</sup> A. Bain, *The emotions and the will*, 3d ed., 1876, 315. Italics mine. See also *The senses and the intellect*, 3d ed., 1874, 300-6; 4th ed., 1899, 322-9.

to voluntary acquisition, as well as the consolidation of instinctive powers, is some force that clenches and confirms a successful chance coincidence. . . . [Parts of the second statement by Spencer, which we have given above, are then quoted.] . . . Here is assumed the law of Pleasure and Pain. Pleasure is accompanied by heightened nervous energy, which nervous energy finds its way to the lines of communication that have been opened up by the lucky coincidence. There is assumed as a consequence, the third of the above postulates—the contiguous adhesion between the two states, the state of feeling, and the appropriate muscular state. The physical expression given by Mr. Spencer to this result, is, I have no doubt, correct—"the opening up of lines of discharge that draw off large amounts of muscular motion."<sup>6</sup>

James Mark Baldwin based his careful treatment of the pleasure-pain theory of learning on the views of both Spencer and Bain. He was principally concerned with the way animals and men accommodate themselves to their environments. He thought that the law of habit which states that "The organism tends to repeat what it has already done" was an inadequate explanation,<sup>7</sup> because, as he said, all movements are not equal before the law of habit. "Painful movements are inhibited, they tend to be reversed, squelched, utterly blotted out; how can this be explained on the foregoing formula for habit?"

Baldwin claimed that "Bain's three postulates [Given above.] . . . touch the inevitable requirements of a theory. . . . For there are three requirements: first, to get movements (his 'spontaneity,' as a substitute for Spencer's 'diffused discharge' and Darwin's 'purposeless contractions'); second, to get selections made from these movements (his 'accidental success,' of certain movements); and third, 'some force that clenches and confirms some successful chance coincidence' ('pleasure and pain,' identified with Spencer's 'heightened nervous energy which finds its way to the lines of communica-

<sup>6</sup> A. Bain, *The emotions and the will*, 3d ed., 1876, 318-9.

<sup>7</sup> J. M. Baldwin, *Mental development in the child and the race; methods and processes*, 1894, 214-20.

tion that have been opened up by the lucky coincidence')."<sup>8</sup> Although Baldwin accepted the principal features of the Spencer-Bain theory of learning, he made a number of reservations, criticisms, and refinements of his own.

"Those movements only are adaptive which secure a *new element of sense process*, such as light, chemical action, food stimulus, etc., in addition to the ordinary advantage of movement itself which all movements, *qua* movements, have in common. . . . Where in the entire series of events constituting a reaction accompanied by pain—stimulus, central process, movement—does the pain come in, *before or after the first adapted movement*, *i.e.*, the movement that has an inhibiting influence somehow upon its own further performance? The whole phraseology of Spencer and Bain would serve to make us think that *it came in only after a movement so unlucky as to be ill-adapted*, the pain being part of the *EFFECT* of the movement, so that, by the memory of the pain thus got, the movement is in future inhibited. The pain got from the movement serves in memory to warn us not to repeat *the movement*. But here I take issue blankly, contending that it comes in *by and in the stimulus and before its discharge in movement*, warning us not to move *so as to repeat that stimulus*. It is by this 'warning,'—which is in organic terms an actual lowering of vitality and consequent *dampening of movement*, or production of contrary movements,—by this the organism tends to avoid the repetition of this stimulation. Let us take for scrutiny the customary illustration—the one which James uses, for example, in explaining the '*Meynert scheme*' of nervous action. A child thrusts his finger in a candle-flame, and is burned: he thrusts no more, but shrinks. Here the doctrine of Spencer, Bain, and many others, seems to make the *function of the pain the inhibition of the thrusting movement*, as itself undesirable. But surely the case is very different. Is this movement in itself undesirable? Is it not undesirable *only under these or similar circumstances*? The *inhibiting effect* and the *pain* are brought about by the *burn*, and the recurrence of that—that is the thing to be prevented. *The thrusting movement is a mere*

<sup>8</sup> J. M. Baldwin, *Op. cit.*, 185. See pp. 170-220.

incident. Suppose the candle is brought up against the child instead of the reverse: it then shrinks just the same. But in this case there has been no forward movement giving a pain, by the memory of which, on the theory in question, the shrinkage or stoppage of thrusting is caused. No doubt the child has a habit of shrinking from pain-causing things; but what I claim is just this, that it is *pain-causing things*, not *painful feeling movements* which it has acquired this habit in reference to. . . . We accept from the Spencer-Bain theory the fact of adaptation by selection from excessive movements, and also the view that the forerunner or cause of these excessive movements is a central process which is the organic analogue of pleasure; (omitting the negative or pain side, which, apart from details, proceeds in a parallel way) but we raise an objection to that theory which seems to us insuperable: The objection that *it makes this pleasure and through it all adaptation, result from one kind of sense-stimulus, that of the organism's own contraction, and not from others*, with no ground whatever for this discrimination against the ordinary stimulations of the environment such as light, heat, oxygen, food-supply, etc., which are most vitally necessary for all growth, from the first.”<sup>9</sup>

The above statement of the views of Spencer, Bain, and Baldwin shows what is not generally recognized, that they deserve the credit for all of the more plausible characteristics of the pleasure-pain theory of learning, and that they are also responsible for most of its undesirable features.<sup>10</sup> In the section which follows we shall describe several apparent limitations of the Spencer-Bain-Baldwin theory, and this discussion may take the place of any treatment of the views of several later exponents of the pleasure-pain theory.<sup>11</sup>

<sup>9</sup> J. M. Baldwin, *Op. cit.*, 191-4. Most of the italics not in the original. Baldwin's reformulation of the law of habit is given in *Op. cit.*, 216-9.

<sup>10</sup> The quotations from Spencer and Bain are fairly representative, but the treatment of Baldwin does not do full justice to his views on imitation.

<sup>11</sup> The views of L. T. Troland (The fundamentals of human motivation, 1928 186-243) will be left out of the later discussion. Troland's theory of learning is based on psychophysical parallelism and the concept of purpose. He pays little attention to the experimental evidence, and states that most of the arguments that have been advanced against the pleasure-displeasure theory are 'exceptionally puerile.' (*Op. cit.*, 203.)

## SECTION 2. GENERAL DEFECTS IN THE SPENCER-BAIN-BALDWIN THEORY OF LEARNING

1. *Much of Our Learning Does Not Involve Adaptation by means of Random Manual Movements.*—The theory described above is concerned with the means by which animals and humans become adapted to their environments, and it involves purposive behavior, biological drives, and beneficial adjustments. A great deal of our learning, however, is not a *biological adaptation* to the environment at all but a kind of *social embroidery* of the present changing civilization. The pleasure-pain theory of learning was described principally in connection with *manual movements*, but it is necessary for a general theory of the learning process to include the large number of important *verbal* and *affective* activities that are also learned. The Spencer-Bain-Baldwin theory does not even cover the learning of *all manual movements*, because many learning situations which involve manual activities are not of the random, trial and error sort.

2. *The Doctrine of Heredity is Incorrect*—Spencer, Bain, and Baldwin were interested in evolution, heredity, adaptation, and the selection of useful movements; and they made the unwarranted assumption that habit grows directly out of instinct. If the animal made a successful adjustment it was a case of the unfolding of nature; but if it did not make a successful adjustment *and died*, the incident could be *passed over lightly* and largely omitted from further consideration. The internal revolution that has overtaken technical genetics in the last few years is of special importance for psychology, and the critical reaction against purposive instincts is by this time familiar to all. The various criticisms directed against instincts apply with even more appropriateness to the unjustified assumption that feelings and emotions are inherited. The supporters of the law of effect have also made the peculiar claim that *all pleasures and pains, all pleasant and unpleasant feelings, and all likes and dislikes are inherited*.

The ability to be pleased or displeased at all may be largely the result of heredity; and native factors may account for the

gross differences in affectivity between men and dogs, on the one hand, and alligators and crocodiles, on the other. It may even be assumed that the differential factors in the production of all of the affectivities are *partly* inherited; but it has now been shown that the *particular stimuli or situations* that will evoke pleasures and displeasures, likes and dislikes, satisfiers and annoyers, and any and all forms of love, anger, fear, pride, and shame, in the *human adult* at least, are *largely learned*.

Inasmuch as the large majority of the annoyances and pleasures of man are largely *learned*, it is necessary to look for a more general explanation of learning than the pleasure-pain theory, in other words one that will also be able to explain how those affectivities that are not inherited are themselves acquired.

The theory that the alleged relation between the pleasant and the beneficial and between the painful and the injurious was established by natural selection, is not in harmony with the facts of modern biology. Among the lower animals the correlation between the pleasant and the beneficial, and between the painful and the injurious, is *far from perfect*, and the correspondence is *quite poor* in the case of man. The use of several drugs is pleasant and injurious, and some diseases are injurious but painless. The attempt to apply the natural selection theory to concrete manual habits is decidedly disappointing. Sandiford, for example, says that "To get one's head under water is originally annoying, consequently the strokes which keep us afloat in swimming are stamped in."<sup>12</sup> If this were the correct explanation of swimming, humans would learn to swim simply by putting their heads under the water, and practically all animals would be natural swimmers. Dogs float and walk in the water, and when a cat is thrown in, he seems to have convulsions.

3. *The Physiological Theory of Pleasure and Pain is Improbable.*—The theory of learning described above assumes that *pleasure* accompanies *increased* nervous activity and that *pain* accompanies *decreased* nervous activity; but experimental data and ordinary observations are opposed to this view. It

<sup>12</sup> P. Sandiford, *Educational psychology; an objective study*, 1928, 197, 199.

is even plausible to believe that the reverse is nearer the truth. As Holmes says, "A child who burns his hands and writhes about in agony certainly manifests a heightened nervous discharge, but he shows no tendency to put his hands again into the fire. . . . The theory of heightened nervous discharge as expounded by Spencer, Bain, and Baldwin, fails to give us, I think, the desired explanation of the acquirement of individual accommodations."<sup>13</sup>

4. *The Pleasure-Pain Philosophy is Inadequate.*—Spencer and Bain overemphasized the importance of pleasures and pains in human learning. Even if one grants that pleasures and pains have *some* influence on the learning process, it is unlikely that they are the *only* factors that have *any* influence. The affectivities are frequently present while learning is taking place, but it is quite probable that the learning process can be adequately described without any special reference to them.

5. *Pleasant and Unpleasant Feelings.*—Some psychologists have objected to the pleasure-pain theory of learning largely because they do not recognize the existence of any conscious processes, and they are even less inclined to admit that pleasant and unpleasant feelings influence behavior. Our own objections to the pleasure-pain theory are of a different nature. Pleasant and unpleasant feelings appear to be *pattern psychological activities*; and they involve factors that are neurological, sensory, muscular, glandular, verbal, conscious, unconscious, and so on, all at the same time. Feelings are *caused* by certain conditions and activities in the organism, and *they are also the causes of other activities*. The causal factors operate in practically all directions between all of the different kinds of processes involved in the total pattern activity. One may hold to the belief that feelings and emotions are of considerable importance in motivating conduct without believing that *they alone* constitute the basic mechanism of the learning process. Affection is not the only factor to be considered. Several different kinds of processes may be present in the organism at the same time, *e. g.*, walking, talking, feeling, breathing, and digesting, but it is unlikely that *any single*

<sup>13</sup> S. J. Holmes, *The evolution of animal intelligence*, 1911, 174-5.

*process* can be the only factor involved in, or the sole cause of, such a general process as learning.

In various discussions of the pleasure-pain theory of learning, there has been a marked tendency to emphasize pleasures more than pains, and the optimistic tendency in judging feelings, of which this is only one aspect, is widespread in the population. It has also been the custom to regard pleasant and unpleasant feelings as the psychological opposites of each other, and to assume that if one effect is produced by a pleasant feeling the opposite effect will be produced by an unpleasant feeling. The greater emphasis on the pleasures seems to be unwarranted, and the opposition between the pleasant and unpleasant feelings is more logical than psychological.<sup>14</sup> There is nothing on the side of pleasure that corresponds in intensity and in persistence to the physical pains. More use is made of pains in medical diagnosis than can be made of pleasures on any occasion. Unpleasant feelings have a more positive and insistent character and they play a more important role in both animals and men. Several arguments have been advanced in support of the tension theory of feelings, which claims that unpleasant feelings frequently involve some kind of strain or tension, and that pleasant activities generally occur only while this tension is being reduced.

### SECTION 3. EXPERIMENTAL DATA ON THE PLEASURE-PAIN THEORY OF LEARNING

1. *Studies of Human Subjects and of the Lower Animals.*—From the somewhat limited point of view of human psychology, the value of studying a subhuman animal depends upon the extent to which the animal in question and man resemble each other in anatomy, physiology, and psychology. The argument by analogy from animals to men becomes decidedly weak at the lower levels of the phylogenetic scale. Historically the pleasure-pain theory of learning has been concerned with the *conscious* activities of *pleasantness* and *unpleasant-*

<sup>14</sup> We have already discussed this subject at some length, in Pleasant and unpleasant feelings, *Psychol. Rev.*, 1930, 37, 228-40.

ness. In the case of man the introspective method is practically indispensable in studying the affectivities, and the dangers of attempting to describe the subjective experiences of animals are of course well known. In spite of these limitations, however, the investigations of the lower animals have an importance in their own right, and certain observations on animals have greatly increased the total interest in the pleasure-pain theory. We shall not attempt to make a clear distinction between the studies of animals and men. The experimental data obtained with human subjects has a more direct bearing on the present topic, but one should consider all of the evidence. If the evidence supports the pleasure-pain theory, then the theory should be graciously accepted, but if it contradicts the theory, then the theory should be abandoned.

2. *The Learning and Retention of Pleasant and Unpleasant Activities.*—The experimental studies that have been made on the 'Relation Between Feeling and Memory' have an important bearing on our present topic. Some of these investigations have been concerned with the relative efficiency of learning the pleasant and unpleasant feelings or emotions themselves, and others have dealt with the relative efficiency of learning different kinds of activities that are P, U, and I.<sup>15</sup> Some of the experiments have been concerned with restricted laboratory material and activities, and these results and conclusions cannot always be transferred to the more general situations of everyday life. Several other studies have dealt with more lifelike situations and activities, and have been carried out at a level that is more likely to yield general conclusions. These studies have employed a variety of activities and materials, and all of the results combined seem to be fairly reliable.<sup>16</sup>

In practically all of these investigations, measures were obtained of the efficiency of learning *P* and *U* activities, and the averages of the two groups of measures were then compared with each other. The results show quite clearly that the over-

<sup>15</sup> Pleasant, unpleasant, and indifferent.

<sup>16</sup> The results of these studies are summarized in the writer's monograph, on The learning and retention of pleasant and unpleasant activities, *Arch. Psychol.*, 1932, 21, No. 134.

*lapping between the two groups of measures is much more significant than the slight and generally unreliable differences between the averages. The small differences between the averages and the large amount of overlapping are sufficient justification for concluding that there is little, if any, difference in the efficiency with which P and U activities can be learned.*

Similar experiments have shown that there is little, if any, difference in the efficiency with which activities that were P and U when they originally occurred can be retained and reproduced. As far as the P and U activities and experiences themselves are concerned, that is, in and of themselves, the U activities can be learned and retained with approximately the same efficiency as the P activities. With equally favorable external and internal conditions, a dislike can be learned as readily as a like, and an annoyance as easily as a pleasure. It should be added, however, that the external and internal conditions of the S's are generally *not equally favorable* for the P and U activities. The factors of desire, interest, and effort favor a relative increase in the efficiency of learning the P activities and a relative decrease in the efficiency of learning the U activities. There is a natural preference for the P activities and a habit of neglecting and trying to avoid the U activities. But if the pleasure-pain theory of learning were true, there would be more difficulty than there is in learning the U activities, and there would be a much greater relative superiority in the efficiency of learning the P activities.

Other investigations have shown that activities and experiences that are decidedly P or decidedly U can be learned more efficiently than activities that are I or only mildly P or mildly U; but that there is little, if any, difference in the efficiency of learning activities and experiences that are mildly P, mildly U, and I.

The customary statements of the pleasure-pain theory, or the law of effect, do not say anything about the fact that *activities that are neither P nor U can be learned*; and those who have supported the theory have not seemed to realize that the U and I activities, the U feelings and the U and I emotions are *often learned in a most efficient manner*. The above considera-

tions would seem to justify the conclusion that the pleasure-pain theory is not a general and all-inclusive law of learning.<sup>17</sup>

3. *Snoddy's Study*.—In Snoddy's study of trial and error learning,<sup>18</sup> objective records and introspections were taken with 9 psychologically trained *S*'s in tracing a 6-pointed star. Snoddy found it easy to divide the sides of the star into the two classes, difficult and easy. The sides that were parallel to the median plane of the body were invariably easy, and with these sides the strength of the muscular tensions reported by the *S*'s were decidedly less than with the other sides. Muscular strains and tensions always seemed to be present when the *S* was tracing a difficult side. On passing from a difficult to an easy side, the unpleasant muscular tensions were followed by a marked relaxation, and this relaxation seemed to be the physical basis of the pleasure that resulted. The sides that were *difficult* and *unpleasant* were the sides where *improvement took place*, and the sides that were *easy* and *pleasant* were the sides where *no improvement was made*. "On those sides of the star. . . where every movement is followed by a feeling of satisfyingness, practically no improvement takes place from the first circuit to the end of practice." The conclusion was reached that the *feeling of satisfyingness* which frequently followed the successful tracing of a difficult side had *no effect upon the learning*.<sup>19</sup>

4. *Morgan's Observations on the Chicks*.—Morgan's observations were prior to Thorndike's earliest studies, and they have been used as illustrations of the pleasure-pain theory of learning almost as frequently as the Meynert scheme of the baby and the candle. In 1896 Morgan observed that domestic chicks without training will peck at anything and every-

<sup>17</sup> If some error has been made in the process of arriving at this conclusion, the writer would be quite pleased to know just where the mistake occurred.

<sup>18</sup> G. S. Snoddy, An experimental analysis of a case of trial and error learning in the human subject, *Psychol. Monog.*, 1920, 28, No. 124.

<sup>19</sup> The results of the study by H. R. Crosland, A qualitative analysis of the process of forgetting, *Psychol. Monog.*, 1921, 29, No. 130, were in a limited way also opposed to the law of effect. Some of the studies on the delayed feeding of animals seem to oppose the law of effect, but the interpretation of these experiments is uncertain. See, for example, J. B. Watson, The effect of delayed feeding upon learning, *Psychobiol.*, 1917, 1, 51-9; and C. J. Warden & E. J. Haas, The effect of short intervals of delay in feeding upon speed of maze learning, *J. Comp. Psychol.*, 1927, 7, 107-16.

thing which is not too large, and which can or cannot be seized, and if possible they will test the object in the bill.<sup>20</sup> He threw to the chicks some distasteful cinnabar larvæ that were conspicuous by alternate rings of black and golden-yellow. The larvæ were seized but dropped, the chicks wiped their bills, and seldom touched these distasteful caterpillars again. The next day the chicks were given some *edible* brown loopers and green cabbage-moth caterpillars, which after some hesitation were eaten freely. When the chicks were then given more of the *distasteful* caterpillars, "One chick ran, but checked himself, and, without touching the caterpillar, wiped his bill—a memory of the nasty taste being apparently suggested by association at sight of the yellow and black caterpillar." Morgan next threw in more *edible* caterpillars, which were eaten freely again. "The chick has thus learnt to discriminate by sight between the nice and nasty caterpillars." The actual *learning* consisted in *inhibiting* the tendency to peck at the *distasteful* black and yellow caterpillars.

In 1901 Hobhouse gave an objective interpretation of the behavior of Morgan's chicks in accordance with his 'Confirmation and Inhibition' theory.<sup>21</sup> He said the 'unpleasant taste' *seemed to have nothing to do with the learning*, and that the 'feelings' (to which he had previously referred) *had no substantial existence*. In 1915, however, he made the confusing statement that "It is the satisfaction or dissatisfaction, to use terms derived from our consciousness, the success or frustration of the conation, to use terms which are more objective, which operates to confirm, modify, or inhibit subsequent conation under similar circumstances."<sup>22</sup>

The most important interpretation of the behavior of the chicks is that which was given by Stout. In 1899 he wrote that "The *sight* of the cinnabar caterpillar re-excites the *total disposition* left behind by the previous experience of *pecking at it, seizing it, and ejecting it in disgust*. Thus the *effect* of these experiences is *revived*. The *sight* of the cinnabar

<sup>20</sup> C. L. Morgan, *Habit and instinct*, 1896, 40-2.

<sup>21</sup> L. T. Hobhouse, *Mind in evolution*, 1901, 85-93.

<sup>22</sup> L. T. Hobhouse, *Mind in evolution*, 2d ed., 1915, 124.

caterpillar has acquired a meaning. It means the experiences which in the first instance followed it; and just because it means them it may more or less dispense with the necessity of actually repeating them. It may so determine the course of action that repetition or re-instatement of the specific items of the previous experience is needless. To this extent, it is practically equivalent to them: it functions instead of them."<sup>23</sup> Stout emphasized the incorporation of new items of experience with those already acquired, and he claimed that the association process affords opportunities for the acquirement of meaning. "If the successive phases of a process concur to form a total disposition as their cumulative effect, the renewal of a part of the process must tend to re-excite this disposition," but "The re-excitement of the cumulative disposition does not necessarily involve revival of the specific items of the previous experience."<sup>24</sup> Stout gave a more accurate description of the learning process than his predecessors, and some of his views closely resemble the later theories of Gestalt psychology and redintegration.

#### SECTION 4. THORNDIKE'S STUDY OF TRIAL AND ERROR BEHAVIOR IN ANIMALS

In Thorndike's original experiments on kittens, dogs, and chicks,<sup>25</sup> the animals were placed in boxes or pens and the time taken to escape was measured on successive trials. The kittens were strongly and uniformly motivated to reach the food box by a state of 'utter hunger,' but the strength of the incentives used for dogs and chicks was different at different times. The behavior of all of the animals was irregular and unpredictable. However, the negative conclusions were drawn that most mammals, barring the primates, do not reason, that they have no power of inference, that they do not imitate except as a result of their native constitution, and that they do not show any association of ideas or have no ideas at all.

<sup>23</sup> G. F. Stout, *A manual of psychology*, 1899, 86. Italics mine.

<sup>24</sup> G. F. Stout, *Op. cit.*, 89. Italics mine. See also 3d ed., 1913, 182-91.

<sup>25</sup> E. L. Thorndike, *Animal intelligence; an experimental study of the associative processes in animals*, *Psychol. Rev., Monog. Suppl.*, 1898, 2, No. 8.

When the animal was placed in one of the embarrassing situations, he seemed to make 'random' movements, one of these movements permitted him to escape, he was rewarded and the resulting pleasure 'stamped in' the 'association.' It was claimed that the *impulses* to successful acts were stamped in by the *pleasure* that followed the acts, and that the *impulses* to unsuccessful acts were stamped out by the *absence of any sequent pleasure*.<sup>26</sup> Thorndike always used the word *pleasure* in this connection.<sup>27</sup> The term *impulse* was defined as "the *consciousness accompanying a muscular innervation*. . . . It is the direct *feeling* of the doing as distinguished from the idea of the act done gained through eye, etc."<sup>28</sup> Thorndike made no mention of the pleasure-pain theory of learning which had been previously described by Spencer, Bain, and Baldwin.<sup>29</sup>

1. *Thorndike's Evidence Against Thinking and Against the Law of Effect in Animals.*—Thorndike was principally interested in the *associative processes* of animals as compared with their abstract, conceptual, and inferential thinking; but some of his evidence *against reasoning* was also strong evidence *against the law of effect*. His conclusions that animals do not reason, and that associations are between sense-impressions and impulse and not between ideas,<sup>30</sup> were based on the general appearance of the learning curves; and this argument can be illustrated best by some of the results with kittens.

In several of these curves there was a sudden decrease in the time required to escape, and then the curves remained down in a very satisfactory manner on later trials. One curve of this type was obtained with kitten No. 12 in box D.<sup>31</sup> Kitten No. 12 took 500 sec. on the *fourth* trial, but the time was only 6 sec. on the *seventh* trial, and it remained below 11 sec. on the remaining 12 trials. This particular curve suggests that there was some kind of mental process in the animal

<sup>26</sup> E. L. Thorndike, *Op. cit.*, 13, 36, 82, 84, 103-4.

<sup>27</sup> See also E. L. Thorndike, *Psychol. Rev.*, 1899, 6, 416-7.

<sup>28</sup> E. L. Thorndike, *Animal intelligence*, *Psychol. Rev., Monog. Suppl.*, 1898, 15. Italics mine.

<sup>29</sup> See, however, E. L. Thorndike, *Op. cit.*, 82, 84, 103.

<sup>30</sup> E. L. Thorndike, *Op. cit.* 71, 66, 73, 74.

<sup>31</sup> E. L. Thorndike, *Op. cit.*, 18.

corresponding to verbal activities or ideas in humans,—or perhaps the *pleasure* had a marked *stamping-in effect*.

In the curve for Kitten No. 2 in box Z,<sup>32</sup> however, the *curve was quite high and irregular for the first 34 trials*. Then for trials 35 to 47 it was relatively low and regular, remaining *below 40 sec.* in all cases. In trials 48 to 63 the time was *above 150 sec.* on 6 occasions. For trials 64 to 73 it remained *below 20 sec.* in all cases. *Then on trail 76 the time increased to 245 sec.!* Such a curve is of course strong evidence against reasoning, because if the kitten had used reason, inference, or ideas, it seems plausible to believe that the time curve would not have been so irregular. *But if the successful movement had become a habit by the pleasure-stamping-in process it is just as reasonable to assume that the habit would have been formed in a much more regular fashion.* Thorndike said that the 'utter hunger' of the kittens made the incentive *uniform in all cases*. But if the hunger was uniform, and the pleasure-stamping-in process was uniform, why were the results so *variable*? If the pleasure-stamping-in principle was a satisfactory explanation of the *short times*, why should this principle fail to operate for the *long times*? *Different kittens gave markedly different curves for the same box, and a large number of kittens failed completely to solve the problems.* The argument offered against reasoning is inconsistent with the explanation of learning in terms of the pleasure-stamping-in process. Thorndike said that "Futile impulses are gradually stamped out. The gradual slope of the time-curve, then, shows the *absence of reasoning*."<sup>33</sup> But the *curves were not gradual*. The large number of *irregular curves* showed the *absence of reasoning*, and also the *absence of any regular pleasure-stamping-in effect*.

The genetic history of Thorndike's animals was not commented upon, and the number and nature of the stimuli that were influencing them were *not known*. The kittens were responding in so many different ways that it would have been impossible for anyone to know what they were actually doing or just what psychological activities were present. The *time*

<sup>32</sup> E. L. Thorndike, *Op. cit.*, 23.

<sup>33</sup> E. L. Thorndike, *Op. cit.*, 45. *Italics mine.*

measures may have been sufficient grounds for believing that certain mental processes were *absent*, but they were not an adequate indication of what psychological activities were *present*. Thorndike's original experiments at least seemed to furnish good evidence that his animals did not learn by any regular functioning of the pleasure-stamping-in principle.

2. *The Factors of Punishment and Reward in Learning.*—

The large number of factors which influence the learning processes of animals include punishment, reward, several native drives, kinaesthetic cues, visual stimuli, external conditions such as temperature, internal conditions such as drugs, and general conditions such as intelligence and age. Like several other factors, punishment and reward may increase the learning activities of animals but the learning process does not have to be described in terms of punishment and reward alone. If this method were used and carried to its logical extreme, we would have a law of learning based on hunger, another law based on sex, and still other laws based on kinæsthetic cues, visual stimuli, and drugs, as well as the law of effect which is based on satisfiers and annoyers.

Punishments and rewards are often *present while learning is taking place*, but they do not for this reason alone constitute the *learning process* itself. A more general and comprehensive law of learning is necessary; a law, in other words, which is independent of the *particular incentives* that are present while learning is taking place. Since animal learning frequently occurs in the absence of a sex drive and also in the absence of punishment and reward incentives, any formulation of the mechanism of learning that is based on *sex alone* or on *punishment and reward alone* is a limited and special case and cannot be regarded as a *general law*. Although punishment and reward are *two factors* that are often present while learning is taking place, and although these incentives frequently influence the learning process, the factors of punishment and reward are not always present in animal learning, and the learning process therefore cannot be described in terms of these two factors alone.

In human subjects learning frequently occurs when the

factors of punishment and reward, and the pleasant and unpleasant feelings, are almost if not entirely absent. One of the most important forms of human learning is concerned with verbal activities, especially those activities which are involved in reading and in listening to another person talking. One generally experiences may different kinds of feelings while he is reading a novel or newspaper, for example, and the feelings may even be unpleasant or indifferent, but associations can still be formed quite rapidly. Associations may be formed when the feelings are pleasant, unpleasant, or indifferent. It is clear that there are other factors in the organism besides the pleasant and unpleasant feelings upon which the formation of associations depends.

#### SECTION 5. THORNDIKE'S STATEMENT OF THE LAW OF EFFECT

Thorndike scarcely mentioned the pleasure-stamping-in theory in his study of monkeys,<sup>34</sup> but in a different publication in 1901<sup>35</sup> he added a discomfort-stamping-out factor, claiming that "Any act which is done in a certain situation and brings discomfort tends to be dissociated from that situation and not to be done again." He seems to have discussed the pleasure-pain theory of learning for the first time in 1908 in an article called *A Pragmatic Substitute for Free Will*, which was included in the Columbia 'Essays Philosophical and Psychological in Honor of William James.' In 1911 he gave a longer discussion of the *law of effect* in his book on *Animal Intelligence; Experimental Studies*.<sup>36</sup> In his earliest writings he had criticized certain animal psychologists because of their *lack of objective experimental evidence*, and he had stressed the necessity of general conclusions always being based on *experimental data*. But his regard for experimental evidence had changed when he reached page 244 of his *Animal Intelligence*, because he stated the 'law' of effect in a very positive way in spite of the fact that there was practically no experimental evidence in its favor.

<sup>34</sup> E. L. Thorndike, The mental life of monkeys, *Psychol. Rev., Monog. Suppl.*, 1901, 3, No. 15.

<sup>35</sup> E. L. Thorndike, The human nature club, 2d ed., 1901, 39.

<sup>36</sup> E. L. Thorndike, *Animal intelligence; experimental studies*, 1911, pp. 244-72.

Thorndike's 1911 statement of the law of effect is so well known that it need not be repeated here.<sup>37</sup> Two years later, in 1913, he added three 'laws' of readiness<sup>38</sup> which have also led to much discussion. We have discussed all of the ideas included in Thorndike's statement of the law of effect in connection with the Spencer-Bain-Baldwin theory of learning. Thorndike permitted the reader to assume that he had originated the law of effect, and he has not been especially inclined to answer his critics.<sup>39</sup>

When Thorndike stated the law of effect in 1911 he rephrased the Spencer-Bain-Baldwin *pleasure-pain* theory of learning which was already familiar. He called his statement the law of *effect*, although the psychological activities involved in it were the *pleasant and unpleasant feelings*. His statement included the concepts of effect, result, and consequence, but it also included satisfiers and annoyers, and annoyances and pleasures. Several modifications of the concept of *effect* have been proposed, such as Carr's 'sensory consequences'<sup>40</sup> and Peterson's 'completeness of response,'<sup>41</sup> but the word 'effect' has always been, and still is, used in the place of 'affect' to designate satisfiers and annoyers, pleasures and annoyances, or pleasant and unpleasant feelings. A modification in the animal's behavior *must* be due to sensory consequences, effects in the central nervous system, or, more accurately perhaps, *results in the whole organism*, but the question about which the discussion has centered is whether *pleasures stamp in* and *annoyances stamp out*. This continues to be a persistent psychological problem.

<sup>37</sup> E. L. Thorndike, *Op. cit.*, 244, 245.

<sup>38</sup> E. L. Thorndike, *The original nature of man*, 1913, 128.

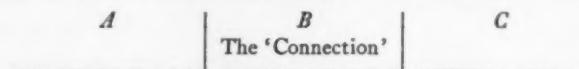
<sup>39</sup> See, for example, J. B. Watson, *Behavior, an introduction to comparative psychology*, 1914, 256-9; S. J. Holmes, *Studies in animal behavior*, 1916, 139-54; and the writer's article, *Criticisms of the laws of exercise and effect*, *Psychol. Rev.*, 1924, 31, 297-417.

<sup>40</sup> H. A. Carr, *Principles of selection in animal learning*, *Psychol. Rev.*, 1914, 21, 157-65.

<sup>41</sup> J. Peterson, *Completeness of response as an explanatory principle in learning*, *Psychol. Rev.*, 1916, 23, 153-62.

## SECTION 6. THORNDIKE'S LATEST VIEWS

Apparently no one has accepted the circular definitions of satisfyingness and annoyingness which Thorndike used in 1911, namely, that satisfyingness is what the animal does and that annoyingness is what it tries to avoid. In 1927 Thorndike stated the law of effect somewhat differently, and referred to "The alleged law of effect, that what comes after a connection acts upon it to alter its strength."<sup>42</sup> It is clearly assumed that the 'connection' extends over a limited period of time because something *comes after it*. In the following diagram, let the passage of time be represented from left to right, and let *A* be *before* the connection, *B* *during* the connection, and *C* *after* the connection. The word 'connection' means, or



should mean, a connection between a situation and a response, that is, a certain kind of activity produced in the *S*'s central nervous system when he is told, for example, to 'draw a line'. During the period *A*, the 'connection' was not an activity, although it may have been a disposition; and during *B*, it is of course an activity. The portion of this activity that is in the central nervous system may continue for a period of time, but sooner or later, after the *S* draws the line, the activity ceases, and when this occurs the 'connection' does not exist any more than a 'knock' in an automobile exists after the 'knock' has ceased or any more than a person's 'walk' exists after he has ceased walking. Something is done to *S* (the 'effect') in the interval *C*. Now if the activity of drawing a line (the 'connection'?) is absent during *C*, or if the 'connection' remains passive and unaffected during *C*, then that is about all there is to say about it. However, if the afferent, central, and efferent activity involved in drawing a line is *active during C*, or is in any way affected during *C*, then the activity in question *extends through the interval C*, and the 'effect' is not entirely retroactive, since the 'connection' is active in the interval *C* while the effect is being produced.

<sup>42</sup> E. L. Thorndike, The law of effect, *Amer. J. Psychol.*, 1927, 39, 212-22.

1. *One of Thorndike's Typical Experiments.*—In one of Thorndike's experiments<sup>43</sup> the *S* was instructed to draw a 3-in. line, and if the line was within 1/8-in. of the correct length, *E* said 'right,' if not, *E* said 'wrong'. The conditions were the same at other times except that *E* did not say anything. The group of *S*'s showed some average improvement when the *E* did not say anything, but they improved more when he said 'right' and 'wrong'. When *E* said 'wrong,' the *S*'s *thought* of the extent of the movement that they had made, and they also *thought* that they must draw longer or shorter lines on later occasions. When *E* said 'right,' they *thought* of the extent of the movement that they had made, and they also *thought* that they must try to draw lines of the same length on later occasions. *E*'s saying 'right' and 'wrong' gave the *S*'s some *instructions* as to how to draw the line and how not to draw it on *later* occasions, it made the *S*'s pay closer attention to what they were doing, and it was the cause of the *S*'s using different methods of making the movement and judging its extent. It was already well known, however, that verbal instructions aid in the performance of many manual activities.

The questions involved in Thorndike's modified statement of the law of effect, that "What comes after a connection acts upon it to alter its strength," may be illustrated in connection with the following diagram.

<i>A</i>	<i>B</i>	<i>C</i>	<i>D</i>	<i>E</i>	<i>F</i>	<i>G</i>
<i>S</i> Draws Line 1	<i>E</i> Says 'Right'	<i>S</i> Draws Line 2	<i>E</i> Says 'Wrong'	<i>S</i> Draws Line 3	<i>E</i> Says Nothing	<i>S</i> Draws Line 4

*S* tries to draw a 3-in. line at *A*, and, at *B*, *E* says 'right.' *S* again tries to draw a 3-in. line at *C*, and at *D*, after Line 2 has been drawn, *E* says 'wrong.' *S* draws a third line at *E*, and then, at *F*, *E* does not say anything. *S* then draws a fourth line at *G*, etc. *S* draws lines at *A*, *C*, *E*, and *G*, and *E* says 'right,' 'wrong,' and nothing at *B*, *D*, and *F*, respectively. The *effect* of *E*'s saying 'right' at *B* is on the *later* drawings of lines, at *C*, *E*, and *G*. It is a logical error to assume that *E*'s saying 'right' at *B* could have any effect on an activity that

<sup>43</sup> E. L. Thorndike, The law of effect, *Amer. J. Psychol.*, 1927, 39, 212-22.

is already over and which no longer exists. Thorndike refers to what happens at *A* as 'a connection,' but *C* is also 'a connection,' and so also is *E*, and *G*. If *E*'s saying 'right' at *B* has any influence at all, it is on the *later drawings* of the line, at *C*, *E*, and *G*, and not on the previous drawing at *A* which is a matter entirely of the past.

2. *An Analogous Illustration.*—Our objections to Thorndike's modified statement of the law of effect may be brought out clearly by the following illustration, which is quite analogous to the experiment described above. Let us suppose that a family lives near a number of stores, *A*, *B*, *C*, *D*, and *E*. The mother says to her child, "Go buy a loaf of bread." The child buys a loaf of bread, and on returning home tells his mother that he bought the bread at store *E*. She says, "I like the bread they carry, always buy it there." Thereafter he always buys it there. Now the mother's telling the child to buy the bread at store *E* did not affect his *previous trip to E* which has already occurred, but it influences where the child will go for bread *on future occasions*. He may eventually forget that his mother ever told him to go to store *E* although he may continue to go there, just as one may forget that he was ever told how to work the gear shift and clutch of an automobile although he may continue to handle them properly. The above considerations do not prove that the pleasure-pain theory of learning is right, and that all other theories of learning are wrong. Thorndike's altered statement of the law of effect is also not justified, as he seems to claim, merely because mental imagery is poor and unreliable, and because the existence of higher mental processes among the lower animals is difficult to demonstrate.<sup>44</sup>

#### GENERAL CONCLUSIONS

The pleasure-pain theory of learning was originated and clearly described by Spencer, Bain, and Baldwin, but their theory is not an adequate explanation of the learning process.

<sup>44</sup>The experiment that we have criticized above seems to be similar to several of the other experiments described in Thorndike's book on Human learning (1931). Several experiments similar to those which Thorndike describes had been previously carried out by others.

Spencer, Bain, and Baldwin were concerned with the way animals adapt themselves to their environments, but much of our learning is not a biological adaptation. They wrote as if feelings and emotions were almost entirely inherited, but in man especially, many of the connections between the situations and the pleasant and unpleasant feelings are learned. Their theory that pleasure accompanies increased nervous activity and that pain accompanies decreased nervous activity is improbable. They stressed the learning of manual movements, but a general theory of learning must also include the learning of verbal and affective activities.

In practically all of the studies that have been made of the relative efficiency of learning pleasant, unpleasant, and indifferent activities, measures were obtained of the average efficiency of learning the three different kinds of activities, and the averages of these three distributions were then compared with one another. The overlapping between the three groups of measures is more significant than the slight and generally unreliable differences between the averages. Unpleasant and indifferent activities are frequently learned in a most efficient manner. The experimental and observational results from both men and animals are opposed to the pleasure-pain theory.

The most widely discussed statement of the pleasure-pain theory of learning is Thorndike's law of effect, but he did not add any new features that had not already been proposed by Spencer, Bain, and Baldwin. In Thorndike's original study of trial and error behavior in kittens, dogs, and chicks, the evidence against the existence of higher mental processes in animals was just as strong evidence against any regular functioning of the pleasure-stamping-in hypothesis. His attempt to define satisfiers and annoyers in objective terms has not been successful, and the alleged law of effect has continued to include the pleasant and unpleasant feelings. His claim that what comes after an activity has a retroactive influence on the activity is a logical error.

Although punishments and rewards are often present while learning is taking place, and although they frequently influence learning, the learning process itself is not identical

with any single incentive or any group of incentives. A general law of learning is necessarily independent of the incentives that may or may not be present while learning is taking place. It seems justifiable therefore to conclude that the pleasure-pain theory of learning is not an adequate explanation or description of the learning process.

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## ADRENALIN AND EMOTION<sup>1</sup>

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We should like to call attention to a doctrine current in American psychology which holds that the injection of adrenalin, while producing the organic state typical of emotion, is nevertheless not capable of producing a genuine emotion. This doctrine is stated rather precisely by Woodworth<sup>2</sup> who, following a summary of the experimental work of Cannon and Marañon on this subject says, "Some of the subjects, after taking the adrenalin," said they felt 'on edge,' as before a game or race in which they were to participate. Some of them reported that they 'felt as if they were afraid, though of course they were not really afraid,' or, 'as if they were awaiting a great joy,' or 'as if they were going to weep without knowing why.' The feelings were all 'as if.'"

Bard<sup>3</sup> phrases the doctrine as follows, "Marañon and others have reported that injections of this substance [adrenalin] into normal human beings in amounts sufficient to evoke these changes did not produce an emotional experience. They merely gave rise to coldly perceived sensations of palpitation, of diffuse arterial throbbing, of oppression in the chest, of trembling, of chilliness, of nervousness. In certain cases these sensations were coldly reminiscent of previous emotional experiences in which such changes had occurred; the subjects described their feelings by such remarks as, 'I feel as if

<sup>1</sup> From the Psychological Laboratory of the N. Y. State Psychiatric Institute and Hospital. We wish to express our thanks to Drs. S. E. Katz, M. H. Harris and E. Brand for advice and assistance in one or another part of this study.

<sup>2</sup> R. S. Woodworth, *Psychology*, Rev. ed., Holt, New York, 1929, 308.

<sup>3</sup> P. Bard, Chap. 12, *The neuro-humoral basis of emotional reactions*, in *The foundations of experimental psychology*, Clark University Press, Worcester, 1929, 480.

afraid,' 'as if moved,' 'as if I had a great fright yet am calm.'" Landis<sup>4</sup> in discussing this same work of Marañon follows essentially the same doctrine but concludes by saying: "Further research could be profitably carried out on this point."

After these statements it is somewhat confusing, upon turning to the work of Cannon,<sup>5</sup> to find him pointing out that in some cases a genuine emotion *can* be produced by the injection of adrenalin. Thus he says<sup>6</sup> when dealing with the experiments of Marañon, "In a smaller number of the affected cases a real emotion developed, usually that of sorrow, with tears, sobs and sighings." These cases may be, as he says, exceptional and due to preparatory emotional sensitization, but since those authors following Cannon have neglected to consider them, they certainly warrant further investigation. The development of the doctrine becomes more involved when we find in another paper that Cannon<sup>7</sup> does not mention the existence of genuine emotional reactions to adrenalin. In this paper delivered during the Wittenberg Symposium, he says: "Further, as Marañon has shown, injections of adrenalin into human beings in amounts which induce the visceral changes characteristic of emotional excitement do not in fact produce an emotional experience; the subject merely becomes reminiscent of other times when these changes were noted—he reports them and remains calm." And again<sup>8</sup> in the discussion following another paper, he says, "It was as if they were sitting by and watching something going on, but the emotion as such was not testified to by these persons." It does not seem that these latter statements can be easily reconciled with the two quoted before. It is perhaps natural under

<sup>4</sup> C. Landis, Chap. 13, *The expression of emotion*, in *The foundations of experimental psychology*, Clark University Press, Worcester, 1929, 511.

<sup>5</sup> W. B. Cannon, *The James-Lange theory of the emotions: A critical examination and an alternative theory*, *Amer. J. Psychol.* 1927, 39, 113.

<sup>6</sup> W. B. Cannon, *Bodily changes in pain, hunger, fear and rage*, 2d ed., Appleton, New York, 1929, 357.

<sup>7</sup> W. B. Cannon, Chap. 22, *Neural organization for emotional expression*, in *Feelings and emotions*, The Wittenberg Symposium, Clark University Press, Worcester, 1928, 266.

<sup>8</sup> W. B. Cannon, Chap. 22, *Neural organization for emotional expression*, in *Feelings and emotions*, The Wittenberg Symposium, Clark University Press, Worcester, 1928, 158.

these circumstances that some misapprehension should have arisen in the literature. However, the source of the confusion lies deeper still.

Direct appeal to the article of Marañon<sup>9</sup> to which Cannon refers shows that in some cases there was a definite and undeniable emotion produced upon the injection of adrenalin. In addition to those cases of 'cold emotion,' the 'as if' type mentioned above, there is sometimes a genuine emotion, as Marañon says "une émotion involontaire complète, c'est-à-dire avec les mêmes éléments somatiques que dans le cas précédent, et en plus la participation psychique affective qui est le complément de ces éléments."<sup>10</sup> The subjects experiencing these emotions feel them as genuine and complete. These cases are the exception in that they occur less often than do the other types, but the mere fact that it is possible to evoke a genuine emotion by reproducing the typical organic state through the injection of adrenalin is a fact of some importance in any theoretical treatment of emotion. Marañon attempts to explain these cases by attributing them to a raised emotional level which is in many cases associated with definite hyperthyroidism. His effort is particularly unconvincing inasmuch as the work of Peabody, Sturgis, Tompkins and Wearn<sup>11</sup> and of Escudero,<sup>12</sup> to whom Marañon refers, shows definitely that the action of adrenalin is far from specific with respect to hyperthyroidism.

When the references which Marañon cites in support of his conclusions are consulted the case becomes still further confused and dubious. In 1920 Marañon<sup>13</sup> gave a certain amount of consideration to these cases of genuine emotion and pointed out that sometimes the organic phenomena may come

<sup>9</sup> G. Marañon, Contribution à l'étude de l'action émotive de l'adrénaline, *Rev. franç. d'endocrinol.*, 1924, 2, 301-325.

<sup>10</sup> G. Marañon, Contribution à l'étude de l'action émotive de l'adrénaline, *Rev. franç. d'endocrinol.*, 1924, 2, 306.

<sup>11</sup> F. W. Peabody, C. C. Sturgis, E. M. Tompkins, & J. T. Wearn, Epinephrin hypersensitivity and its relation to hyperthyroidism, *Amer. J. Med. Sci.*, 1921, 161, 508-517.

<sup>12</sup> P. Escudero, La prueba de la adrenalina en el diagnóstico del hipertiroidismo, *Semana médica*, 1921, 28, 116-117.

<sup>13</sup> G. Marañon, La reacción emotiva a la adrenalina, *Med. ibera*, 1920, 12, 353-357.

first and then secondarily give rise to the psychic emotion. Sierra,<sup>14</sup> whom Marañon quotes as confirming his work, mentions enthusiastically that the injection of adrenalin may produce a true emotional crisis and says that Marañon was the first to notice the rich psychic content that often accompanies the physiological symptoms of the adrenalin syndrome. He hails Marañon as having given to psychology an instrument for the experimental study of emotion that is practical, economical, inoffensive, and sure. Avedillo<sup>15</sup> mentions that Marañon has succeeded in some cases in provoking emotion by adrenalin without the initial intervention of a central psychic factor. In neither of these references does there seem to be any idea of a necessity of explaining a way or minimizing the examples of genuine emotion following the injection of adrenalin.

In 1922 we find Marañon<sup>16</sup> definitely stating that the emotional predisposition responsible for these cases is a more or less intense state of hyperthyroidism, a statement which he carefully qualifies in his 1924 article. One gets the impression that although the physiological and psychological findings following the injection of adrenalin have been constant, Marañon's interpretation of these events has shifted, without his having made plain the fact that there has been such a change. It is very difficult to judge the adequacy of Marañon's theoretical conclusions since only a minimum of actual experimental data has ever been published.

After going over the data presented in the original Spanish articles we can only conclude that there are at least two major psychological syndromes resulting from the injection of adrenalin. In the first, which constitutes a majority of the cases, the somatic disturbances are reported and regarded as 'as if' or 'cold' emotion. In the second group, the subjects

<sup>14</sup> A. M. Sierra, Estudio psicológico acerca de la emoción experimental, *Rev. de criminol., psiquiat., y med. leg.*, 1921, 8, 445-461; Estudio psicopatológico referente a la emoción experimental, *Semana méd.*, 1921, 28, 225-232.

<sup>15</sup> A. L. Avedillo, Correlación hormónica de las manifestaciones fisopatológicas de la emoción, *Med. ibera*, 1921, 15, 49.

<sup>16</sup> G. Marañon, Problemas actuales de la doctrina de las secreciones internas, Ruiz Hermanos, Madrid, 1922, 155-159.

report an experience which to them constitutes a real and valid emotion.

Recently, Cantril and Hunt<sup>17</sup> have reported upon the emotional effects of the intramuscular injection of 1.5 cc. of adrenalin chloride into normal individuals. They concentrated upon the psychological approach, finding three types of reaction, (1) the mere report of the organic symptoms of the syndrome; (2) the association of the present state with a previous emotional state (the 'cold' or 'as if' emotions of Marañon); and (3) the report of a genuine emotion. These last cases were very few, but seemed no less definite and real for that reason. While they agree that their results are in line with those of Marañon, they differ with him over the interpretation of the cases of true emotion, holding that they cannot be satisfactorily explained by predisposition or raised emotional level.

Probably a certain part of the confusion existing in this field may be attributed directly to a confusion in the meanings which are attached to the term 'emotion.' We propose, therefore, to set up the following distinctions with regard to the meaning of the word 'emotion.' '*Emotion-subjective*' will be used to describe experience reported by the subject which may or may not be open to observation or recording by an observer. The nature of this subjective experience is of course extremely variable and is open to doubt or reinterpretation from the standpoint of objective psychology. However, the person having the experience is quite certain of its existence, although the components of the experience may vary from time to time.

'*Emotion-objective*.'—By this phrase we mean the outward and manifest expressive emotional reactions which may be noted and agreed upon by observers. These reactions are open to instrumental recording of one or another variety. There is no necessity that emotion-subjective and emotion-objective occur simultaneously.

'*Emotion-social*.'—By this we mean types of behavior aris-

<sup>17</sup> H. Cantril & W. A. Hunt, Emotional effects produced by the injection of adrenalin, *Amer. J. Psychol.*, 1932, 44, 300-307.

ing out of more or less complicated social situations which by general consent are characterized as emotional. There is no necessity for simultaneity between emotion-social and either emotion-subjective or emotion-objective.

The present investigation was planned so that the relationship existing between reactions following the injection of adrenalin and emotion might be studied and that the previous work of Cantril and Hunt might be amplified. It was felt that the use of a group of psychopathological individuals representing a wide sampling of the various mental disorders would check Marañon's results with similar cases and at the same time add to the scanty data available in his reports. Since it is commonly held that the emotional life of the psychopathological patient is usually disorganized we hoped that the effects of adrenalin would show themselves in an exaggerated degree. Practically every investigator in this field has mentioned the fact that the true emotional reactions to the injection of adrenalin might be in part explained on the basis of either hyperthyroidism, or on the basis of psychoneurotic personality. We hoped that by using such a group we might obtain more truly emotional reactions and that if such reactions were obtained they might supplement and be compared to the findings of Cantril and Hunt with normal subjects. It was also planned to observe any stable relationships that might exist between blood pressure, pulse rate, and the adrenalin reaction; to study the relationship of the adrenalin syndrome to the various psychoses and finally to investigate in a preliminary way the relationship to experimental hyperthyroidism.

#### TECHNIQUE

Intramuscular injections in the upper arm of 0.5, 1.0 and 1.5 cc. of 1-1000 solution of Parke, Davis Adrenalin Chloride were alternated with control injections of 0.5 and 1.0 cc. of normal physiological salt solution (0.8% NaCl). Injections were given on successive days except when a week-end intervened. Thus a typical series would occupy five days and would consist of injections of 0.5 cc. adrenalin on the first day, 0.5 cc. saline on the second, 1.0 cc. adrenalin on the third,

1.0 cc. saline on the fourth, and 1.5 cc. adrenalin on the fifth. The order of administration of the adrenalin and control solutions were alternated with different subjects. The subject was seated comfortably on a *chaise-longue* and allowed to rest and compose himself for a few minutes before the experiment began. His pulse and blood-pressure were then taken and a note made of his general condition. Then the injection was given. Immediately following this and at brief intervals thereafter determinations of the pulse and blood pressure were made. Record was also kept of the objective manifestations of the adrenalin syndrome, such as mydriasis, tremor, possible sweating, the local skin sign, the behavior of the patient, and of any subjective reports he might make in answer to the question, "How do you feel?"

After the first six patients, the series was shortened by omitting the 0.5 cc. injections of both adrenalin and saline, as 0.5 cc. of adrenalin was not considered sufficient to give a fair test. About this time, the control solution was also changed.

The so-called normal physiological salt solution has been the standard control used in previous experiments. For psychological purposes it is not wholly satisfactory, as the experimenters found their subjects noticing the difference between the saline and adrenalin solutions. The adrenalin chloride produces a slight smarting and stinging immediately upon injection which the saline solution does not. It was advisable therefore, to find a solution which also stings and smarts and yet introduces no complicating factors. A solution of  $2\frac{1}{2}$  grains of chloretone in 1 ounce of physiological salt solution, which is the same as the standard Parke, Davis solution of 1-1000 adrenalin chloride with the adrenalin missing, was made up and used. This strength of chloretone is not sufficient to produce any untoward effects. This control solution is an excellent one as it causes the same sensations and immediate irritation to the tissues as adrenalin. We feel that this chloretone solution is a reliable control, particularly where psychological factors are apt to complicate the problem.

In addition to this routine, it seemed desirable to tenta-

tively explore the effects of adrenalin after the administration of thyroid. Seven patients were given thyroid extract and then subjected to the adrenalin routine once more. Standard desiccated thyroid in tablet form was used, one grain of this being equivalent to 5 grains of fresh gland. Three male catatonics were given 3 grains a day for 3 days and four of the women were given 6 grains a day for 3 days. Of the women two were selected because they had shown clear 'emotion-objective' reactions to adrenalin, and two who had no known emotional reactions to adrenalin. The adrenalin routine was started in every case on the fourth day following the first injection of thyroid. The basal metabolic rate (BMR) was available on about half of the patients used in this study. This rate was considered in the interpretation of the results.

In all, 27 subjects were used, 19 women and 8 men. They were distributed among the various psychoses as follows: 4 were diagnosed as manic depressive, manic type; 2 manic depressive, depressed type; 5 dementia praecox simple form; 4 dementia praecox, catatonic form; 3 dementia praecox, paranoid form; 2 general paresis; 5 psychopathic personality; 1 involutional melancholia; 1 post-influenzal toxic psychosis.

#### RESULTS

Since most previous investigators have given only 'typical' illustrations it seems best to give a brief protocol for each case. In general it may be stated that in every case of the injection of adrenalin the physical symptoms of the syndrome were present. In no case did these appear after the injection of the control solution. The case summaries of general and emotional behavior follow:

1. Dementia praecox, catatonic form; male; age 22; Basal Metabolic Rate (BMR), -3%: 0.5 cc. adrenalin—doubtful increase in general activity: 0.5 cc. saline solution—no change: 1.0 cc. adrenalin—doubtful increase in activity: 1.0 cc. saline—no change: 1.5 cc. adrenalin—increase in activity, patient much livelier (non-emotional).

2. Dementia praecox, catatonic form; male; age 20; BMR, -8%: 0.5 cc. saline—no change: 0.5 cc. adrenalin—increase in

activity: 1.0 cc. saline—no change: 1.0 cc. adrenalin—no change: 1.5 cc. adrenalin—increase in activity.

3. Dementia praecox, catatonic form; male; age 21: 0.5 cc. adrenalin—no change: 0.5 cc. saline—no change: 1.0 cc. adrenalin—"I feel lousy, I feel sick," no other indication of emotion: 1.0 cc. saline—no change: 1.5 cc. adrenalin—increased liveliness and less mental confusion.

4. Dementia praecox, catatonic form; female; age 34: 1.0 cc. chloretone control solution—no change: 1.0 cc. adrenalin—"I feel nervous," no other indication of emotion: 1.5 cc. adrenalin—no change.

5. Dementia praecox, simple form; female; age 17; BMR, -11%; 1.0 cc. adrenalin—no change: 1.0 cc. chloretone—no change: 1.5 cc. adrenalin—"I feel nervous and upset."

6. Dementia praecox, simple form; female; age 25: 0.5 cc. saline—no change: 0.5 cc. adrenalin—"I feel nervous": 1.0 cc. saline—no change: 1.0 cc. adrenalin—"I feel a trifle shaky": 1.5 cc. adrenalin—pronounced muscular twitching.

7. Dementia praecox, simple form; female; age 17; BMR, -1%: 0.5 cc. adrenalin—no change: 0.5 cc. saline—no change: 1.0 cc. adrenalin—scolding of the experimenter, due wholly to the natural critical attitude the subject habitually observed toward the experimenter: 1.0 cc. saline—some giggling (emotion-objective): 1.5 cc. adrenalin—no change.

8. Dementia praecox, simple form; female; age 35: 1.0 cc. adrenalin—"I feel concerned, very anxious" (emotion-subjective): 1.0 cc. chloretone—no change: 1.5 cc. adrenalin—"I feel as if something were radically wrong. I am getting very nervous" (emotion-subjective).

9. Dementia praecox, simple form; female; age 26; BMR, -22%: 1.0 cc. adrenalin—"I feel pretty nervous": 1.0 cc. chloretone—no change: 1.5 cc. adrenalin—hysterical laughter (emotion-objective).

10. Dementia praecox, paranoid form; male; age 14: 1.0 cc. adrenalin—"I am getting too excited," noticeable increase in general excitement: 1.0 cc. chloretone—no change: 1.5 cc. adrenalin—"I am getting too nervous," increase in general excitement.

11. Dementia præcox, paranoid form; male; age 22: 1.0 cc. adrenalin—great deal of apprehension exhibited (emotion-subjective and emotion-objective): 1.0 cc. chloretone—no change: 1.5 cc. adrenalin—"I feel weak and trembly."
12. Dementia præcox, paranoid form; male; age 44: 1.0 cc. chloretone—no change: 1.0 cc. adrenalin—no change. (Patient left hospital after the second injection.)
13. Manic depressive, manic phase; female; age 34, BMR, + 2%: 1.0 cc. adrenalin—"I feel nervous." Severe manic attack of customary character followed, typical of patient and not traceable to adrenalin: 1.0 cc. chloretone—slight manic attack: 1.5 cc. adrenalin—"I feel as if I had more life."
14. Manic depressive, manic phase; female; age 17; BMR, - 1%: 1.0 cc. adrenalin—"I feel nervous": 1.0 cc. saline—no change: 1.5 cc. adrenalin—"I feel nervous. Feel rotten."
15. Manic depressive, manic phase; female; age 35; 1.0 cc. saline—no change: 1.0 cc. adrenalin—doubtful increase in excitement: 1.5 cc. adrenalin—doubtful increase in excitement.
16. Manic depressive, manic phase; female; age 26; BMR, - 15%: 1.0 cc. saline—no change: 1.0 cc. adrenalin—crying spell (emotion-objective): 1.5 cc. adrenalin—hysterical laughter (emotion-objective).
17. Manic depressive, depressive phase; female; age 48: 1.0 cc. adrenalin—hysterical laughter (emotion-objective): 1.0 cc. saline—no change: 1.5 cc. adrenalin—increased excitement and talkativeness.
18. Manic depressive, depressive phase; female; age 45; BMR, - 3%: 1.0 cc. adrenalin—weeping spell (emotion-objective and emotion-subjective): 1.0 cc. saline—no change: 1.5 cc. adrenalin—no change.
19. Psychopathic personality; female; age 27; BMR, + 5%: 0.5 cc. saline—no change: 0.5 cc. adrenalin—no change: 1.0 cc. saline—no change: 1.0 cc. adrenalin—no change: 1.5 cc. adrenalin—"I feel my pulse all over my body."
20. Psychopathic personality; female; age 62; BMR, + 5%: 1.0 cc. saline—no change: 1.0 cc. adrenalin—"I throb all over," increased hypochondriacal complaining: 1.5 cc. adrenalin—increased hypochondriacal complaining.

21. Psychopathic personality; female; age 28; 1.0 cc. saline—no change: 1.0 cc. adrenalin—"I feel nervous": 1.5 cc. adrenalin—"I feel nervous."

22. Psychopathic personality; female; age 20; BMR, - 13%: 1.0 cc. adrenalin—crying and whimpering (emotion-objective and emotion-subjective): 1.0 cc. saline—no change: 1.5 cc. adrenalin—"I feel nervous," crying and whimpering (emotion-objective and emotion-subjective).

23. Psychopathic personality; female; age 23; BMR, + 3%: 1.0 cc. chloretone—no change: 1.0 cc. adrenalin—"I feel funny inside": 1.5 cc. adrenalin—"I feel nervous."

24. General paresis; male; age 35; BMR, + 8%: 1.0 cc. adrenalin—"I feel all right": 1.0 cc. chloretone—no change: 1.5 cc. adrenalin—"I feel all right."

25. General paresis; male; age 42; BMR, - 12%: 1.0 cc. saline—no change: 1.0 cc. adrenalin—"I think this stuff braces you up": 1.5 cc. adrenalin—unusual volubility and excitement (non-emotional).

26. Involutional melancholia; female; age 40; 1.0 cc. chloretone—no change: 1.0 cc. adrenalin—fidgeting and scowling: 1.5 cc. adrenalin—fidgeting and scowling.

27. Post-influenzal toxic psychosis; female; age 22; 1.0 cc. adrenalin—whimpering and crying; following the first injection the patient left the hospital (emotion-objective and emotion-subjective).

It is evident from these protocols that in only one case did the injection of control solution result in anything approaching either emotion-objective or emotion-subjective and that was merely a mild giggling, which was more or less habitual with that patient. We can therefore safely conclude that the emotional manifestations produced by the injection of adrenalin were directly attributable to that agent and are not to be attributed to suggestion. Emotional reactions, either subjective or objective were few, and in general the psychological reactions to adrenalin were either missing or if they did occur they were covered by such a mild statement as, "I feel nervous." Nevertheless, there were a few definite cases of weeping or laughing which clearly show that the adrenalin was sufficient to produce or start a genuine emotion-objective.

The material given in the protocols has been tabulated in tables 1, 2, and 3. In table 1 we have related the appearance or non-appearance of emotional reactions to the clinical syndromes. It is clear that the appearance of these emotional reactions was not correlated positively with any of the classifications used with this group. Changes in systolic blood pressure and in pulse rate are related to the occurrence of emotional reactions in table 2. This shows that the emotional reactions were in every case associated with an increase in systolic blood pressure, in all but one case with an increase in pulse rate, and never with a decrease in systolic blood pressure. This argues for the necessity of an organic basis in the development of either emotion-objective or emotion-subjective. The relation between BMR and emotional reactions shown in table 3 is particularly interesting since the positive emotional reactions which were obtained with the 15 patients on which we had BMR's, occurred in all but one instance with the lowest metabolic rates, namely - 13, - 15, and - 22. This is exactly contrary to the hypothesis that hyperthyroidism is the sole or chief factor giving rise to these adrenalin reactions.

As we have said before, in no case did the control solution produce the characteristic adrenalin syndrome with blanching, tremor, etc. In all our experiments the patients reacted to the injections of adrenalin with some of the general physiological characteristics of this syndrome. The individual features of the syndrome, however, showed extreme variability; the only constant features being the appearance of the local skin reactions, the general blanching, and the appearance of a general tremor in the extremities. In most cases the systolic blood pressure rose, as did the pulse; but the rise was not constant either between individuals or in the same individual. In a very few cases, a depressor reaction occurred with a drop in pulse and systolic blood pressure.

Cases 1, 2 and 3 (male catatonics), who were given 3 grains of thyroid a day for 3 days and then repeated the adrenalin routine, showed no increase in susceptibility to adrenalin after the thyroid treatment, either emotionally or in such

TABLE I  
RELATION BETWEEN THE CLINICAL SYNDROME AND THE APPEARANCE OR NON-APPEARANCE OF EMOTIONAL REACTIONS FOLLOWING THE INJECTION OF ADRENALIN (DISCREPANCIES BETWEEN NUMBER OF INJECTIONS AND TOTAL OF COLUMNS DUE TO FACT THAT ONE INJECTION SOMETIMES GAVE RISE TO BOTH EMOTION OBJ. AND EMOTION SUBJ.)

No. of patients .....	Dementia praecox			Manic depressive insanity			Psychopathic personality			General paroxysms			Involunt. melan-choly			Post-influenza toxic psychosis			Total		
	Cata-tonia	Simple	Paranoic	Manic	Depressive	Manic	Depressive	Manic	Depressive	Manic	Depressive	Manic	Depressive	Manic	Depressive	Manic	Depressive	Manic	Depressive	Manic	Depressive
4	5	3		4	2		5		2	1	1		1		1	1	1	1	1	27	
Emotion obj. to 0.5 or 1.0 cc. adrenalin.....				1	1	2	1		1	1			1		1		1		1	6	
Emotion obj. to 1.5 cc. adrenalin.....				1	1	1	1		1	1			1		1		1		1	3	
Emotion subj. to 0.5 or 1.0 cc. adrenalin.....				1	1	1	1		1	1			1		1		1		1	5	
Emotion subj. to 1.5 cc. adrenalin.....				1	1	1	1		1	1			1		1		1		1	2	
Increased nervousness or activity of non-emotional character, to adrenalin .....	8	6	3	6	1	1	7		2	2			35								
No change to adrenalin .....	3	3	1	1	1	2	2		2	2			12								
No change to control solution .....	7	6	3	4	2	6	2		2	1			31								
Emotion obj. or emotion subj. to control solution .....		1											1								

TABLE 2

RELATIONSHIP BETWEEN CIRCULATORY VARIATIONS AND APPEARANCE OR NON-APPEARANCE OF EMOTIONAL REACTIONS (DISCREPANCIES IN TOTALS ON THIS TABLE AND WHEN THIS TABLE IS COMPARED TO TABLE 1 ARE DUE TO THE FACT THAT SOME INJECTIONS FAILED TO GIVE EITHER A BLOOD PRESSURE OR A PULSE CHANGE AND WERE NOT INCLUDED IN THIS TABLE).

	Systolic blood pressure						Pulse rate	
	Increase			Decrease			Increase	Decrease
	0-5 mm.	5-10 mm.	above 10 mm.	0-5 mm.	5-10 mm.	below 10 mm.		
No. of injections.....	7	12	40	17	4	4	57	24
Emotion obj. to 0.5 or 1.0 cc. adrenalin.....		1	5				6	
Emotion obj. to 1.5 cc. adrenalin.....		1	2				2	1
Emotion subj. to 0.5 or 1.0 cc. adrenalin.....		1	5				6	
Emotion subj. to 1.5 cc. adrenalin.....		1					1	
Increased nervousness or activity of non-emotional character, to adrenalin.....	5	5	24	1			31	4
No change to adrenalin.....	1	3	7				12	
No change to control solution.....	1	1	1	16	4	4	5	19
Emotion obj. or emotion subj. to control solution.....			1				1	

TABLE 3

RELATION BETWEEN BASAL METABOLIC RATE (BMR) AND APPEARANCE OR NON-APPEARANCE OF EMOTIONAL REACTIONS AFTER INJECTION OF ADRENALIN

BMR	Number of patients	Emotion-objective to adrenalin	Emotion-subjective to adrenalin	Emotion-subjective or objective to control solution	Increased nervousness or non-emotional activity to adrenalin	No change to either adrenalin or control solution
+8.....	1					3
+5.....	2				3	5
+3.....	1				2	1
+2.....	1				2	0
-1.....	2				3	4
-3.....	2	1	1	1	3	4
-8.....	1				2	3
-11.....	1				1	2
-12.....	1				2	1
-13.....	1	2	2			1
-15.....	1	2				1
-22.....	1	1			1	1
Total.....	15	6	3	1	19	26

physiological reactions as blood pressure, etc. Cases 5 and 23, selected as typical of those patients giving no known emotional reactions, also showed no reliable difference in physical susceptibility after the administration of 6 grains of thyroid a day for 3 days, and failed also to show any signs of an emotional reaction to adrenalin after this treatment. Cases 16 and 22, who had previously shown undeniable emotional reactions to adrenalin, were also given 6 grains of thyroid a day for 3 days. They showed no increased physical susceptibility to this agent, neither did they exhibit any emotional reactions such as they had shown previously. In every case the thyroid, despite its apparently negative results in relation to adrenalin, had produced distinct and noticeable increases in the general level of activity of the patients while on the ward.

#### DISCUSSION

We have shown that there is a doctrine current which states that the injection of adrenalin is not of itself sufficient to give rise to a genuine emotion. When the sources quoted in support of this doctrine were investigated, we found that they state clearly that in some cases a true emotion may be aroused by the injection of adrenalin. Marañon does not attribute such 'true emotion' to the simple reproduction of the organic syndrome by adrenalin, but states that it is probably due to hyperthyroidism, 'predisposition,' or 'raised emotional level' on the part of the subject. In our experiments a majority of the injections gave rise to increased 'nervousness' (non-emotional) which might be construed as the 'cold' or 'as if' emotions of Marañon. Only in a few cases were complete 'emotions' evoked by adrenalin. In spite of the fact that these cases were not in a majority, we see no reason for explaining them by reference to 'predisposition' or 'raised emotional level.' These terms are at best vague and lacking in definite context. Our data on psychopathological patients are, in our opinion, directly comparable to those of Cantril and Hunt<sup>18</sup> with normal subjects; especially since there seems

<sup>18</sup> H. Cantril & W. A. Hunt, Emotional effects produced by the injection of adrenalin, *Amer. J. Psychol.*, 1932, 44, 300-307.

to be no reliable differences between the two groups with respect to the physiological reactions to adrenalin. [The introspections obtained by these latter writers show clearly that in some instances the mere existence of the organic syndrome set up by adrenalin was accepted as furnishing a complete and satisfying subjective emotional experience. It is of course extremely difficult to get satisfactory verbal reports of an introspective nature from psychopathological cases, but there was every overt indication that these were genuine 'emotion-objective' or 'emotion-subjective' reactions and that they may be interpreted in the same fashion as those reported by Cantril and Hunt. It might have been expected that, due to psychopathological conditions, our subjects would present the physiological adrenalin reactions in an exaggerated form. Such was not the case. Our results were remarkably similar to those obtained by Cantril and Hunt with normal individuals.

The experiments in which we used thyroid were very limited, both from the standpoint of the amount of the agent given and from the number of subjects used. For such reason we are not in a position to draw any conclusion, but we do want to call attention to the fact that in the patients with which we worked we were unable to find any increase in the effect of the adrenalin after the administration of thyroxin although the behavior of the 'thyroxin' patients while on the wards indicated that the thyroxin was having some effect. As table 2 indicated, we found greater emotional reactions in patients with low metabolic rates. Again we do not feel that any conclusions are justified other than noting that in this limited sample the expected correlations were reversed. These results are in accordance with the work of Peabody, Sturgis, Tompkins and Wearn,<sup>19</sup> who found that the close correlation between hyperthyroidism and the adrenalin reaction (which Marañon felt would explain these 'true emotions') was not definite. Moreover, the conclusion of Meyer,<sup>20</sup> that

<sup>19</sup> F. W. Peabody, C. C. Sturgis, E. M. Tompkins, & J. T. Wearn, Epinephrin hypersensitivity and its relation to hyperthyroidism, *Amer. J. Med. Sci.*, 1921, 161, 508-517.

<sup>20</sup> M. Meyer, Zur Frage der Adrenalinunempfindlichkeit bei Dementia Praecox, *Monatschr. f. Psychiat. u. Neurol.*, 1917, 41, 24-33.

the dementia praecox patient is relatively insensible to adrenalin was not confirmed by our results. No one of our 12 schizophrenic cases exhibited this insensitivity. Nor can we agree with McWilliam<sup>21</sup> that the reaction to adrenalin corresponds to the degree of emotional instability or emotional defect. The relation between the adrenalin syndrome and emotional stability of these patients shows no such simple function in our experiments.

Taken together with the previous work of Cantril and Hunt on normal subjects, our work indicated that the relation of adrenalin to emotion may be summed up somewhat as follows. In general, the injection of sufficient amounts of adrenalin will reproduce roughly the organic picture usually characterized as emotion. There is a considerable variation, both quantitative and qualitative, within the specific details of this picture. The production of this organic state characteristic of the adrenalin syndrome gives differing results. Some subjects report merely the organic syndrome. Some report an associated emotional content, saying they feel 'as if afraid' or 'as if in great joy,' etc. In a few cases a complete emotion is present, and this emotion seems satisfying and genuine. In our few cases, thyroid treatment improved neither the bodily nor the emotional susceptibility to the adrenalin. The various mental disorders showed no difference in susceptibility to the physical effects of adrenalin.

The bearing of these results on contemporary theories of emotion is equivocal. One can claim that the cases of genuine emotion aroused by adrenalin support the James-Lange theory. They do. One can also claim that the relatively small number of these cases opposes the James-Lange theory. It does. It is the contention of the writers that the James-Lange theory is inadequate to present-day knowledge, and further, that the subject of emotion is a much more complex one than many writers and investigators realize. Certain investigators appear to feel that definite and stable neural patterns of a thalamic or diencephalic level can be found to

<sup>21</sup> W. McWilliam, The sensitivity of the sympathetic nervous system to adrenalin in some cases of mental disorder, *J. Ment. Sci.*, 1925, 71, 432-438.

account for emotion. These patterns undoubtedly do exist, although their stability and constancy is probably only relative. If one stopped here, arbitrarily defining these objective patterns as emotions, the problem would be solved. However, to proceed further and to say that these behavior patterns form the physiological basis for the subjective experience of the felt emotion is, at the best, conjectural. The 'felt' emotion is a more variable and less tangible entity than any of these relatively simple neurological or behavior patterns. As Landis <sup>22</sup> has pointed out, environmental factors undoubtedly are reflected in and form an important part of the emotional experience. The introspections of Cantril and Hunt confirm this. They found most of their observers demanding a satisfactory reason for the emotion before the emotional experience could be felt as complete. The writers therefore suggest that the emotional experience (particularly emotion-subjective) is a highly variable state in which higher intellectual processes play an important part. The felt emotion often partakes of the complicated nature of a judgment. Thus in some individuals the mere eliciting of the organic emotional state (upset) by adrenalin may be sufficient, and the individual will accept it as an emotion. Other more critical individuals, however, will demand the presence of additional factors before accepting the experience as truly emotional. In short, as James and Cannon have pointed out, the emotional *awareness* must be considered as a process involving higher perceptual or intellectual functions.

Our results are limited in scope but despite this it is dubious that the present formulation of the doctrine of imbalance of the crano-sacral-sympathetic divisions of the autonomic nervous system furnishes the explanation for the affective specificity of pleasant and unpleasant emotional experience. The adrenalin syndrome (supposedly the correlate of sympathetic action) may be the basis for either pleasant or unpleasant emotional experiences.

In conclusion we may say that while adrenalin furnishes an

<sup>22</sup> C. Landis, Studies of emotional reactions, II, General behavior and facial expression, *J. Comp. Psychol.*, 1924, 4, 496.

interesting and suggestive approach to the problems of emotion, it is by no means as perfect an instrument as the Spanish school has enthusiastically proclaimed. However the present experiment suggests several lines of further work which might be profitably investigated. The muscular tremor produced by adrenalin injection should be studied and its relation to emotional tremor investigated. Marañon's claims concerning the effect of suggestion and hypnosis on adrenalized states should be tested with proper controls. It would be exceedingly interesting to find out what further stimuli, emotional or non-emotional, could be added to the adrenalin syndrome to produce a genuine emotion; also what might be the effect of adrenalin on emotional states already under way. Finally, the lack of uniformity in the results reported by all investigators of this problem would seem to indicate that there is something peculiar to some individuals with respect to their reactions to the injection of adrenalin. These peculiarities are interesting and deserve careful attention.

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## THE PSYCHOPATHOLOGY OF TIME<sup>1</sup>

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Considerable interest has been stimulated recently in the psychopathology of time. This is an old psychiatric field but a relatively unexplored one. Despite psychiatric routing examinations of space and time orientation, there seems to be very little objective information (14) about time orientation, time estimation, behavior with respect to time. Some of the recent writers, under an undirect influence of Bergson (24), relativity, phenomenology (13, 20, 31), have ventured rather deeply into this unknown realm. Time and space, it is even maintained, are basic and of primary significance in most neuroses and psychoses. A general view of various recent contributions may be afforded by a brief survey of Janet, Minkowski, Straus, Gebssattel, and Fischer.

### I. SURVEY OF RECENT CONTRIBUTIONS

In 1928, Janet (12) set forth a theory of the evolution of memory and of the notion of time. Time and memory are considered as social products. At one stage in the development of man, no difference was known between the past, present, and future. Social necessity, however, caused man to label various time situations and actions. Janet may be said to have projected recently various aspects of his previous contributions upon the screen of time. For him, actions, sentiments, reality, and *le récit* are to be taken together. Inability or failure to label or react to the remote past as remote past, to the present as present and so on, results in various time illusions and disorientations common in neuroses. Janet's old case of Irene, who repeatedly relived the death-scene of her mother illustrates undue influence of the past

<sup>1</sup> Paper read at meeting of the New York Branch of the American Psychological Association, University of Pennsylvania, April 9, 1932.

in the present. Madeleine, wont to prophesy future events, was blind to the past and present. A 28-year old patient mourned deeply for her mother—dead twenty-five years. Janet's (11, 12) main contentions center upon the over- and under-estimation of *le récit*: upon illusions originating in an incorrect labelling of time situations, upon incorrect analysis of situations in terms of degree of reality, in terms of the past present, and future.

According to E. Minkowski (27), space and time disorders underlie most psychoses. He believes that in Senile Dementia there is a memory disturbance, but that the dynamic *I-Here-Now* factors are intact; that in Schizophrenia, the memory is intact and the *I-Here-Now* is disturbed. In Senile Dementia, the future is completely subordinated to the past. Confabulations of these patients are freely interspersed with time expressions as if a defective memory were compensated for (25). Depressed patients feel that their future is completely and fatally hopeless. General Paralytics look towards and even make fantastic plans for the future. Schizophrenic patients may say that they feel left behind as the world moves on. One individual said that the time in his room was a hour behind the time in the next room. In another case, the patient shot at a clock, his worst enemy. A schizophrenic complained of the immobility of everything about him, of the mechanical routine which he followed and which seemed to turn his future into a sheer repetition of the past. Another patient, persuaded he could never achieve a serious goal, tried to get rid of the notion of time. Minkowski asserts that in *syntonia* there is absence of striving, that one is then in touch with reality (world-time), and that in *schizoidia*, one strives and is out of contact with reality (world-time) (17, 18, 19, 21, 22, 23, 26).

In 1928, Straus (30) followed up Minkowski's analysis of Melancholia with a theoretical discussion in which he developed a theory of this psychosis. Time disorders were assumed to be basic to various melancholic delusions and compulsory ideas. While a feeling of joy may result from anticipation of possibilities for development in the future,

a depressed reaction may result from feeling that there is little chance for future personal development, which feeling inhibits reactions in the present situation.

Gebssattel in 1928 reported a case of melancholia in which the patient regarded everything as bringing him nearer the day of his death. The patient drew a line to represent time and made arrows pointing to and from the supposed future day of his death. Suicide is discussed, by Gebssattel (6).

In 1929, Fischer (2, 3, 4, 5) first presented a series of schizophrenia cases later followed up by another series and by theoretical discussion. He also stresses the *I-Here-Now*. His patients make the following complaints: a feeling that the head is a clock; that time comes to a standstill; that the standstill of time is compensated for by a dreamlike existence; dæmonic playing with time; a prayer for the destruction of time; living in eternity; a feeling of timelessness. Fischer supposes that there is no schizophrenic symptom which cannot be a time-space disorder.

Viewed logically, these theories and clinical analyses are quite certain to contain many a loophole and omitted step. No clear connection has been established between space-time disorders and psychomotor and affective functions. It seems ultra-hypothetical to put major stress on space and time. No controls are applied. No statistical analysis is thought of. Janet refers to the belief of psychiatrists in 1890 that the *déjà-vu* illusion (paramnesia) occurred in 50 percent or in 30 percent of clinical cases. He says that in his own 4- or 5,000 observations he came across no more than a dozen such cases. A perpendicular drop from 50 percent or 60 percent to less than 0.3 percent.

## II. SOCIAL PSYCHOLOGY AND PSYCHOPATHOLOGY OF TIME

In the light of my own preliminary experiments in the social psychology of time, the constructive side of the above contributions would seem to lie in the suggestions for research. The following series<sup>2</sup> of time reactions and attitudes may be culled from the reports just reviewed:

<sup>2</sup> See Pichon's classification (28).

1. *Wrongly labelled time-situation reactions.*
2. *Euphoric reaction:* optimism about the future.
3. *Catastrophic reaction:* the future viewed as a series of tragic events.
4. *The prophetic reaction:* utter disregard of the past and present.
5. *One-sided planning reaction:* a plan once made must be carried out in spite of the unforeseen and the unexpected (15, 16, 29).
6. *Syntonic reaction:* akin to extroversion, keeping in time with the outside world time.
7. *Schizoid reaction:* akin to introversion, autistic time, out-of-time with the world about.
8. *Superiority-inferiority reactions:* as pictured by Dodge and Kahn (1), attempts to hold on to the present, fear of losing the past, squandering other individuals' time.
9. *Suicide reaction:* escape from time.
10. *Death-attitudes:* attitudes to death.

In 1930, in a paper entitled *Some aspects of the social psychology of futurism* (7), I attempted to suggest varieties of social attitudes to and interests in the future. Since then, a series of preliminary experiments on time attitudes and reactions (8) has been conducted. In one experiment (9), college students by method of paired comparison indicated for themselves the relative importance of the past, present, and future. It was found that the present and future are almost of equal importance to the students. Statistically speaking, the present was regarded 1.2 times as important as the future, and 12.7 times as important as the past. Data, in another experiment, indicated that the subjects were emotionally oriented towards the present and future. The past was of little emotional value. Another experiment (10) with some psychopathological aspects dealt with wishes concerning future events assumed to be improbable. The experiment was suggested by others in this series. Subjects stated for each one of ten given situations three most important things which they wanted to see happen but which they were most

certain would *never* happen. For the various groups of subjects, the greatest convergence of opinion was obtained for *International Affairs*. The subjects agreed that the three most important things they wanted to see happen in *International Affairs* which they were most certain would never happen are: no more wars (according to 53.2 percent of the subjects), complete understanding between nations (33.3 percent), common language (23.8 percent). This is perhaps a study of utopian wishes, regarded commonly as beyond realization. The psychoanalyst might add that the aim of this experiment is to reveal basic conflicts between fantastic wishes and reality. It seems to suggest a way to measure social and individual time perspective limits as well as improbable futuristic wishes.

There is apparently a close connection between social and abnormal time reactions and attitudes.

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## DISCUSSION

### THE ADRENAL CORTEX AND EMOTION: A REPLY

The recent article 'Discussion of "The Adrenal Cortex and Emotion"' by Lester S. King (6) would seem to indicate that our original article (5) was far from clear in its meaning, or at least that certain misconceptions were present in regard to it. Upon carefully re-reading the article, it would seem that in all probability the cause of the confusion lies in the following statement: "Hammett even brought forth his results as a proof that mental states can affect bodily constitution, apparently overlooking the fact that the variability of the nitrogenous materials might affect the reactions of the individual."

It is well to state as a preliminary correction that the authors did not mean by this statement that the variation in the non-protein nitrogen (N. P. N.) of the blood was *the* cause of emotional reactions. This meaning appears to have been read into it. It would be quite possible that there would be no causal relationship found between two events which are correlated; but the usual course of events is that there is more or less causal connection in such cases. However, a belief that a single factor was controlling in such a complex situation as an emotional response would be exceedingly presumptuous.

It was and is our belief that there are numerous changes in the body in emotional states, some of which are known, and more (in all probability) which are unknown as yet. It would seem possible that a relation between the adrenal cortex and the N. P. N. of the blood is one of them. The extent to which it is a factor, if it is a factor, is as yet unknown.

With this in mind, it seems that three of King's five objections are without adequate foundation. His first, third, and fourth objections are various phases of an objection which might be stated simply by noting that there is not a direct correspondence between the N. P. N. and the emotional state. Thus in his first objection he states, "But this is no ground for correlating a given emotional state and a given N. P. N. level, such as the theory under criticism requires." In the third he says that "The N. P. N. is removed from the blood by the kidneys. Rise in the N. P. N. level in the blood

beyond normal limits is considered an indication of kidney damage. Such pathological increases, with or without emotional counterparts, must find a place in the theory under discussion." The fourth objection is that while it may be possible that the adrenal cortex exerts some action on the kidneys, still "To neglect all other functions, and to dispel the cloud of mystery by a bold assumption concerning non-protein nitrogen, is scientifically not wise."

We have never said, and certainly have had no idea of implying, that the only factor influencing emotion was the N. P. N., nor that the only function of the adrenal cortex was to regulate the N. P. N. We believe that it is possible that the functioning of the adrenal cortex in emotional states will produce a *change* in the N. P. N. level of the blood; such change being from the previous level, whatever it may be.

Our position may be made more clear by an analogy to the action of adrenin. In conditions of excitement the increased production of adrenin acts to liberate sugar stored in the liver. It thus *changes* the level of the blood sugar. This change is not, however, to any fixed value which determines the emotional expression. Moreover, in the presence of pancreatic lesions, producing diabetes mellitus, the blood sugar level is markedly shifted even under normal conditions; but still, as far as we know, the values are *changed* by the action of adrenin.

The abnormal N. P. N. values due to pathological kidney conditions presumably have the same significance. If we assume that a lowered N. P. N. level is of adaptive value, then an abnormally high value following kidney injury would indicate a less adaptive functioning of the organism. This seems to be the case.

It also is to be noted that the available evidence indicates that the cortex is slower in its reactions than is the medulla. Cannon, who has played an important role in the demonstration of adrenin as an important factor in emotional states, has also shown that the production of adrenin is too slow to account for the 'felt' emotion (3). If this be true, then it is obvious that any attempt to state that 'a given emotional state and a given N. P. N. level' must be correlated is obviously contrary to facts.

King's second objection is that "In muscular activity there is, under normal conditions, 'practically no change' (1) in the nitrogen output. There is thus no evidence that the N. P. N. level would be significantly raised. There is no advantage in lowering the N. P. N. level in normal blood."

This statement overlooks more recent experimental findings. Parnas and Mozolowski (9) and Embden (4) find that ammonia is given off in muscular work. Parnas states that "muscular work always, without exception, is associated with ammonia production" (8). The ammonia so produced is presumably changed to urea in the liver, since Benedict and Nash do not find an increased amount of ammonia in the urine as would be expected under such conditions (2).

This leads to interesting speculative possibilities. Nash and Benedict have shown that the production of urea from ammonia is reversible (7). Now it is well known that in a reversible reaction, the removal of one of the products tends to rapidly displace the equilibrium in the direction of the removed substance. If, then, the adrenal cortex be assumed to lower the level of the N. P. N. of the blood by excreting it in the urine in the form of urea (King notes that the "N. P. N. is removed from the blood by the kidneys"), then the ammonia from the muscle metabolism would more rapidly be converted into urea. The utility of such a possibility is to be seen in the fact that the protein of the diet is largely utilized by the process of converting the protein to amino-acids, which are in turn changed to glycogen as a final product, with ammonia given off in the process of deaminization. The production of ammonia in the muscle can be interpreted only as such a change, a part of the amino-acids there being utilized in the form of glycogen, with the production of ammonia. Assuming a lower-than-normal level of urea in the blood, the free ammonia of the blood would more rapidly be converted to urea, and therefore the ammonia of the muscle would more rapidly be removed from the cells. (The direction and rate of flow of a substance through a semi-permeable membrane such as a cell wall is largely affected by the relative concentrations of the substance on the two sides.) As a final step in this hypothesis, it is clear that such removal of ammonia would enable the cell to more rapidly split additional amino-acids and obtain further glycogen, thus producing much improved functioning.

*It is not to be assumed that the authors believe that this is the final functioning of the adrenal cortex, or that this hypothesis will be shown to be absolutely true. Other theories, possibly equally plausible, might be advanced. This one seems possible from our present knowledge, but has been presented simply to show that King's statement that "The phrase 'clearing the blood of impurities'*

is meaningless" may be, all things considered, somewhat premature. The removal of the N. P. N. of the blood quite possibly has more or less effect upon subsequent actions. Only further research can determine this.

His fifth objection is that we are inconsistent with our own sources of information. Thus "When some of these other men flatly contradict the theory in question, that theory cannot be considered of great value. Thus, Hollingshead and Barton say, 'The medullary portion (of the adrenal gland) . . . and the cortical portion . . . would be called into action simultaneously.' But Wyman and Walker say, 'Much of the recent information concerning the physiology of the suprarenal cortex points towards a steady maintenance of certain bodily conditions by some influence from that organ. By a steady maintenance it is implied that the influence from the cortex is constantly acting, possibly requiring appreciable time to produce its effects, and is not a factor which lies in reserve ready to be called upon (10).'"

It appears that King did not see or fully consider the implications of the last part of the same paragraph, which was, "The action of the other part of the gland, the suprarenal medulla, in supplying influences to readjust certain bodily equilibria when called upon in emergency conditions is well known." Thus Wyman and Walker were not stating that the functioning of the cortex was inflexible, but that it was relatively inflexible as compared with the medulla. The evidence would seem to justify such a conclusion. But the cortical secretion is no more uniform than the medullary secretion can be said to be an emergency function; there is normally a continuous functioning of both portions, and in periods of emotion or excitement the medulla would probably act more rapidly than the cortex. This is to be expected from the fact that the medulla is more richly innervated than is the cortex, but this is not equivalent to saying that the cortical portion does not secrete more rapidly as a part of the emotional expression than when some less emotional configuration is present. There is as yet no evidence indicating that any organ is independent of its connections to the nervous system; even the advocates of the myogenic theory must admit that at times the heart is acted upon by the nervous system, regardless of whether the normal contraction is initiated in the heart.

King confuses immediacy of stimulation of the organ with immediacy of reaction to the hormone. There are probably at least two rea-

sons for the apparent lag in cortical response as compared to the medullary: the first is the comparatively small increase, already pointed out, the second is probably intrinsic in the nature of the cortical hormone, since experiments show that injections of the hormone do not produce maximal reactions for a period of four to five hours. An assumed function of the cortex in emotion would therefore be to emotional conditions of several hours duration.

In conclusion, of King's five objections, the first, third, and fourth seem to be due to a misconception of our meaning. We did not intend to imply that there would be found an actual correlation of any emotion with a given N. P. N. level. Our statement was that it appeared that the cortex was capable of producing a change in the N. P. N. level. The second objection would seem to be due to a lack of knowledge in regard to recent experimental findings. The fifth objection we believe to be the result of oversight of, or lack of consideration in regard to, the last part of the material quoted.

King also objects to the fact that we have done no work on the theory ourselves. As a partial defense it may be stated that we did not feel that we had adequate facilities for the preparation of the hormone, and that upon writing to Parke, Davis and Co. in November, 1930 we were informed that although Drs. Swingle and Pfiffner were working in connection with them, as yet no satisfactory commercial extract was available. Latest advices are that the extract is still too scarce to be used experimentally. It seemed to us that publication of the hypothesis might possibly stimulate someone possessing more adequate facilities to work on the problem. Failing that, and possibly in any case, we will undertake experiments as soon as materials are adequately available.

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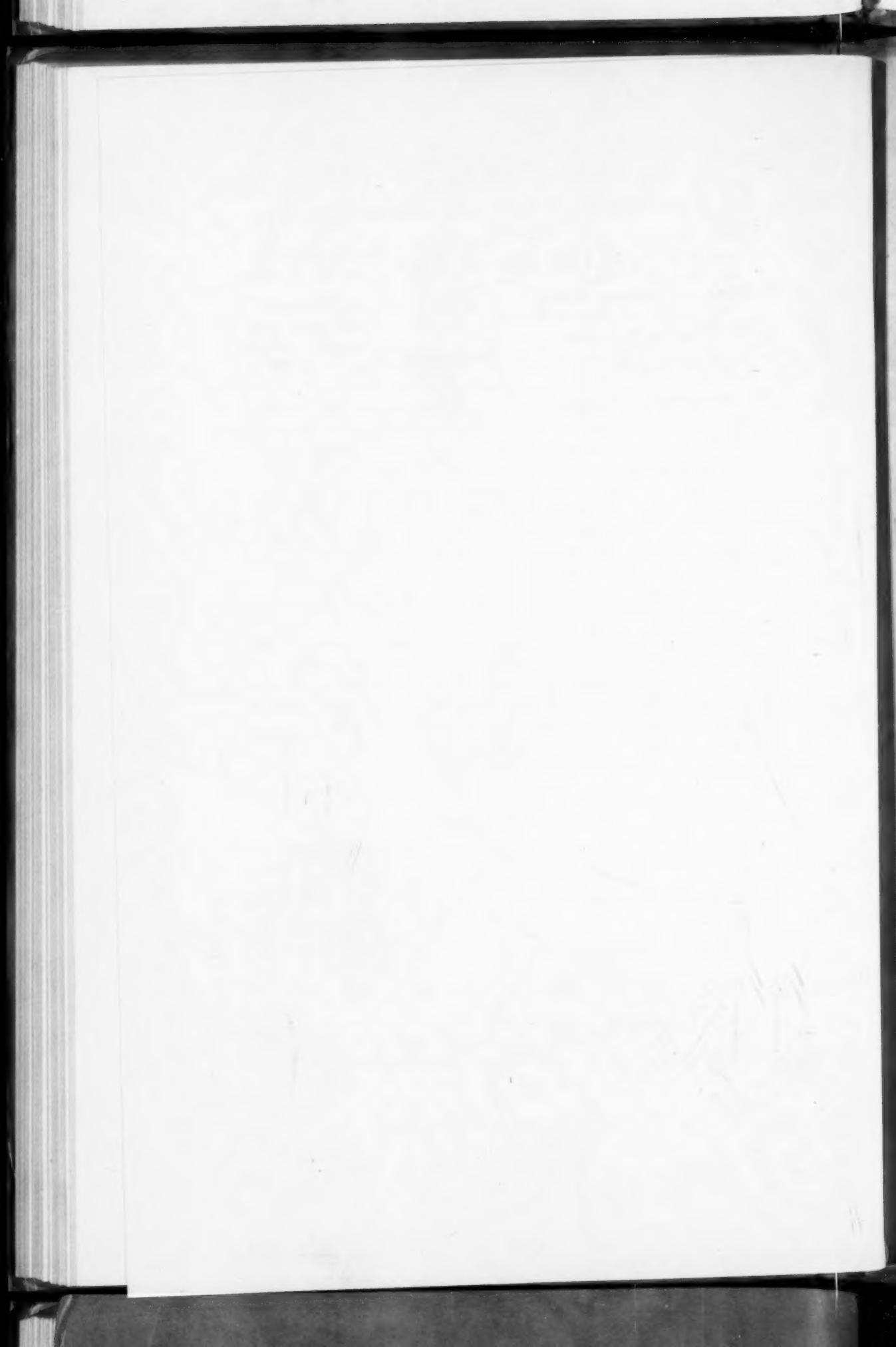
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